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I.—WITTGENSTEIN'S LECTURES IN 1930-33

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I

IN January, 1929, Wittgenstein returned to Cambridge after an absence of more than fifteen years. He came with the intention of residing in Cambridge and pursuing there his researches into philosophical problems. Why he chose Cambridge for this latter purpose I do not know: perhaps it was for the sake of having the opportunity of frequent discussion with F. P. Ramsey. At all events he did in fact reside in Cambridge during all three Full Terms of 1929, and was working hard all the time at his researches.[1] He must, however, at some time during that year, have made up his mind that, besides researching, he would like to do a certain amount of lecturing, since on 16th October, in accordance with his wishes, the Faculty Board of Moral Science resolved that he should be invited to give a course of lectures to be included in their Lecture List for the Lent Term of 1930.

During this year, 1929, when he was researching and had not begun to lecture, he took the Ph.D. degree at Cambridge. Having been entered as an "Advanced Student" during his previous period of residence in 1912 and 1913, he now found that he was entitled to submit a dissertation for the Ph.D. He submitted the *Tractatus* and Russell and I were appointed to examine him.

[1] The statement in the Obituary notice in *The Times* for 2nd May, 1951, that he arrived in Cambridge in 1929 "for a short visit" is very far from the truth. Fortunately I kept a brief diary during the period in question and can therefore vouch for the truth of what I have stated above about his residence in 1929, though there is in fact other evidence.

We gave him an oral examination on 6th June, an occasion which I found both pleasant and amusing. We had, of course, no doubt whatever that his work deserved the degree: we so reported, and when our report had been approved by the necessary authorities, he received the degree in due course.

In the same month of June in which we examined him, the Council of Trinity College made him a grant to enable him to continue his researches. (They followed this up in December, 1930, by electing him to a Research Fellowship, tenable for five years, which they afterwards prolonged for a time.)

In the following July of 1929 he attended the Joint Session of the Mind Association and Aristotelian Society at Nottingham, presenting a short paper entitled "Some Remarks on Logical Form". This paper was the only piece of philosophical writing by him, other than the *Tractatus*, published during his life-time. Of this paper he spoke in a letter to MIND (July, 1933) as "weak"; and since 1945 he has spoken of it to me in a still more disparaging manner, saying something to the effect that, when he wrote it, he was getting new ideas about which he was still confused, and that he did not think it deserved any attention.

But what is most important about this year, 1929, is that in it he had frequent discussions with F. P. Ramsey—discussions which were, alas! brought to an end by Ramsey's premature death in January, 1930.[1] Ramsey had written for MIND (October 1923, p. 465) a long Critical Notice of the *Tractatus*; and subsequently, during the period when Wittgenstein was employed as a village-schoolmaster in Austria, Ramsey had gone out to see him, in order to question him as to the meaning of certain statements in the *Tractatus*. He stayed in the village for a fortnight or more, having daily discussions with Wittgenstein. Of these

[1] In the Preface to his posthumously published *Philosophical Investigations*, where Wittgenstein acknowledges his obligations to Ramsey (p. x), Wittgenstein himself says that he had "innumerable" discussions with Ramsey "during the last two years of his life", which should mean both in 1928 and in 1929. But I think this must be a mistake. I imagine that Wittgenstein, trusting to memory alone, had magnified into a series of discussions continuing for two years, a series which in fact only continued for a single year. It will be noticed that in the letter from Ramsey himself which I am about to quote, and which is dated 14th June, 1929, Ramsey states that he had been in close touch with Wittgenstein's work "during the last two terms", *i.e.* during the Lent and May terms of 1929, implying that he had not been in close touch with it in 1928. And though I do not know where Wittgenstein was in 1928, he certainly was not resident in Cambridge where Ramsey was resident, so that it is hardly possible that they can have had in that year such frequent discussions as they certainly had in 1929.

XUM

discussions in Austria I only know that Ramsey told me that, in reply to his questions as to the meaning of certain statements, Wittgenstein answered more than once that he had forgotten what he had meant by the statement in question. But after the first half of the discussions at Cambridge in 1929, Ramsey wrote at my request the following letter in support of the proposal that Trinity should make Wittgenstein a grant in order to enable him to continue his researches.

" In my opinion Mr. Wittgenstein is a philosophic genius of a different order from any one else I know. This is partly owing to his great gift for seeing what is essential in a problem and partly to his overwhelming intellectual vigour, to the intensity of thought with which he pursues a question to the bottom and never rests content with a mere possible hypothesis. From his work more than that of any other man I hope for a solution of the difficulties that perplex me both in philosophy generally and in the foundations of Mathematics in particular.

" It seems to me, therefore, peculiarly fortunate that he should have returned to research. During the last two terms I have been in close touch with his work and he seems to me to have made remarkable progress. He began with certain questions in the analysis of propositions which have now led him to problems about infinity which lie at the root of current controversies on the foundations of Mathematics. At first I was afraid that lack of mathematical knowledge and facility would prove a serious handicap to his working in this field. But the progress he has made has already convinced me that this is not so, and that here too he will probably do work of the first importance.

" He is now working very hard and, so far as I can judge, he is getting on well. For him to be interrupted by lack of money would, I think, be a great misfortune for philosophy."

The only other thing I know about these discussions with Ramsey at Cambridge in 1929 is that Wittgenstein once told me that Ramsey had said to him " I don't like your method of arguing ".

Wittgenstein began to lecture in January ,1930, and from the first he adopted a plan to which he adhered, I believe, throughout his lectures at Cambridge. His plan was only to lecture once a week in every week of Full Term, but on a later day in each week to hold a discussion class at which what he had said in that week's lecture could be discussed. At first both lecture and discussion class were held in an ordinary lecture-room in the University Arts School; but very early in the first term Mr. R. E. Priestley (now Sir Raymond Priestley), who was then

University Registrar and who occupied a set of Fellows' rooms in the new building of Clare, invited Wittgenstein to hold his discussion classes in these rooms. Later on, I think, both lectures and discussion classes were held in Priestley's rooms, and this continued until, in October, 1931, Wittgenstein, being then a Fellow of Trinity, was able to obtain a set of rooms of his own in Trinity which he really liked. These rooms were those which Wittgenstein had occupied in the academic year 1912-13, and which I had occupied the year before, and occupied again from October, 1913, when Wittgenstein left Cambridge and went to Norway. Of the only two sets which are on the top floor of the gate-way from Whewell's Courts into Sidney Street, they were the set which looks westward over the larger Whewell's Court, and, being so high up, they had a large view of sky and also of Cambridge roofs, including the pinnacles of King's Chapel. Since the rooms were not a Fellow's set, their sitting-room was not large, and for the purpose of his lectures and classes Wittgenstein used to fill it with some twenty plain cane-bottomed chairs, which at other times were stacked on the large landing outside. Nearly from the beginning the discussion classes were liable to last at least two hours, and from the time when the lectures ceased to be given in the Arts School they also commonly lasted at least as long. Wittgenstein always had a blackboard at both lectures and classes and made plenty of use of it.

I attended both lectures and discussion classes in all three terms of 1930 and in the first two terms of 1931. In the Michaelmas term of 1931 and the Lent term of 1932 I ceased, for some reason which I cannot now remember, to attend the lectures though I still went to the discussion classes; but in May 1932, I resumed the practice of attending the lectures as well, and throughout the academic year 1932-33 I attended both. At the lectures, though not at the discussion classes, I took what I think were very full notes, scribbled in note-books of which I have six volumes nearly full. I remember Wittgenstein once saying to me that he was glad I was taking notes, since, if anything were to happen to him, they would contain some record of the results of his thinking.

My lecture-notes may be naturally divided into three groups, to which I will refer as (I), (II) and (III). (I) contains the notes of his lectures in the Lent and May terms of 1930; (II) those of his lectures in the academic year 1930-31; and (III) those of lectures which he gave in the May term of 1932, after I had resumed attending, as well as those of all the lectures he gave in the academic year 1932-33. The distinction between the three

groups is of some importance, since, as will be seen, he sometimes in later lectures corrected what he had said in earlier ones.

The chief topics with which he dealt fall, I think, under the following heads. First of all, in all three periods he dealt (A) with some very general questions about language, (B) with some special questions in the philosophy of Logic, and (C) with some special questions in the philosophy of Mathematics. Next, in (III) and in (III) alone, he dealt at great length, (D) with the difference between the proposition which is expressed by the words " I have got tooth-ache ", and those which are expressed by the words " You have got tooth-ache " or " He has got tooth-ache ", in which connexion he said something about Behaviourism, Solipsism, Idealism and Realism, and (E) with what he called " the grammar of the word ' God ' and of ethical and aesthetic statements ". And he also dealt, more shortly, in (I) with (F) our use of the term " primary colour " ; in (III) with (G) some questions about Time ; and in both (II) and (III) with (H) the kind of investigation in which he was himself engaged, and its difference from and relation to what has traditionally been called " philosophy ".

I will try to give some account of the chief things he said under all these heads ; but I cannot possibly mention nearly everything, and it is possible that some of the things I omit were really more important than those I mention. Also, though I tried to get down in my notes the actual words he used, it is possible that I may sometimes have substituted words of my own which misrepresent his meaning : I certainly did not understand a good many of the things he said. Moreover, I cannot possibly do justice to the extreme richness of illustration and comparison which he used : he was really succeeding in giving what he called a " synoptic " view of things which we all know. Nor can I do justice to the intensity of conviction with which he said everything which he did say, nor to the extreme interest which he excited in his hearers. He, of course, never read his lectures : he had not, in fact, written them out, although he always spent a great deal of time in thinking out what he proposed to say.

(A) He did discuss at very great length, especially in (II), certain very general questions about language ; but he said, more than once, that he did not discuss these questions because he thought that language was the subject-matter of philosophy. He did not think that it was. He discussed it only because he thought that particular philosophical errors or " troubles in our thought " were due to false analogies suggested by our actual use of expressions ; and he emphasized that it was only necessary

for him to discuss those points about language which, as he thought, led to these particular errors or "troubles'.

The general things that he had to say about language fall naturally, I think, under two heads, namely (*a*) what he had to say about the meaning of single words, and (*b*) what he had to say about "propositions".

(*a*) About the meaning of single words, the positive points on which he seemed most anxious to insist were, I think, two, namely (α) something which he expressed by saying that the meaning of any single word in a language is "defined", "constituted", "determined" or "fixed" (he used all four expressions in different places) by the "grammatical rules" with which it is used in that language, and (β) something which he expressed by saying that every significant word or symbol must essentially belong to a "system", and (metaphorically) by saying that the meaning of a word is its "place" in a "grammatical system".

But he said in (III) that the sense of "meaning" of which he held these things to be true, and which was the only sense in which he intended to use the word, was only one of those in which we commonly use it: that there was another which he described as that in which it is used "as a name for a process accompanying our use of a word and our hearing of a word". By the latter he apparently meant that sense of "meaning" in which "to know the meaning" of a word means the same as to "understand" the word; and I think he was not quite clear as to the relation between this sense of "meaning" and that in which he intended to use it, since he seemed in two different places to suggest two different and incompatible views of this relation, saying in (II) that "the rules applying to negation actually describe my experience in using 'not', *i.e.* describe my understanding of the word", and in one place in (III), on the other hand, saying, "perhaps there is a causal connection between the rules and the feeling we have when we hear 'not'". On the former occasion he added that "a logical investigation doesn't teach us anything about the meaning of negation: we can't get any clearer about its meaning. What's difficult is to make the rules explicit".

Still later in (III) he made the rather queer statement that "the idea of meaning is in a way obsolete, except in such phrases as 'this means the same as that' or 'this has no meaning'", having previously said in (III) that "the mere fact that we have the expression 'the meaning' of a word is bound to lead us wrong: we are led to think that the rules are responsible to something not a rule, whereas they are only responsible to rules".

As to (α) although he had said, at least once, that the meaning

of a word was " constituted " by the grammatical rules which applied to it, he explained later that he did not mean that the meaning of a word *was* a list of rules ; and he said that though a word " carried its meaning with it ", it did not carry with it the grammatical rules which applied to it. He said that the student who had asked him whether he meant that the meaning of a word *was* a list of rules would not have been tempted to ask that question but for the false idea (which he held to be a common one) that in the case of a substantive like " the meaning " you have to look for something at which you can point and say " This is the meaning ". He seemed to think that Frege and Russell had been misled by the same idea, when they thought they were bound to give an answer to the question " What *is* the number 2?" As for what he meant by saying that the meaning of a word is " determined by " (this was the phrase which he seemed to prefer) the " grammatical rules " in accordance with which it is used, I do not think he explained further what he meant by this phrase.

(β) As to what he meant by saying that, in order that a word or other sign should have meaning, it must belong to a " system ", I have not been able to arrive at any clear idea. One point on which he insisted several times in (II) was that if a word which I use is to have meaning, I must " commit myself " by its use. And he explained what he meant by this by saying " If I commit myself, that means that if I use, *e.g.* ' green ' in this case, I have to use it in others ", adding " If you commit yourself, there are consequences ". Similarly he said a little later, " If a word is to have significance, we must commit ourselves ", adding " There is no use in correlating noises to facts, unless we commit ourselves to using the noise in a particular way again—unless the correlation has consequences ", and going on to say that it must be possible to be " led by a language ". And when he expressly raised, a little later, the question " What is there in this talk of a ' system ' to which a symbol must belong ? " he answered that we are concerned with the phenomenon of " being guided by ". It looked, therefore, as if one use which he was making of the word " system " was such that in order to say that a word or other sign " belonged to a system ", it was not only necessary but *sufficient* that it should be used in the same way on several different occasions. And certainly it would be natural to say that a man who habitually used a word in the same way was using it " systematically ".

But he certainly also frequently used " system " in such a sense that *different* words or other expressions could be said to belong to the *same* " system " ; and where, later on, he gave, as

an illustration of what he meant by "Every symbol must essentially belong to a system", the proposition "A crotchet can only give information on what note to play in a system of crotchets", he seemed to imply that for a sign to have significance it is *not* sufficient that we should "commit ourselves" by its use, but that it is also necessary that the sign in question should belong to the same "system" with other signs. Perhaps, however, he only meant, not that for a sign to have *some* meaning, but that for *some* signs to have *the significance which they actually have in a given language*, it is necessary that they should belong to the same "system" with other signs. This word "system" was one which he used very very frequently, and I do not know what conditions he would have held must be satisfied by two different signs in order that they may properly be said to belong to the same "system". He said in one place in (II) that the "system of projection" by which "2 + 3" can be projected into "5" is "in no way inferior" to the "system" by which "11 + 111" can be projected into "11111", and I think one can see, in this case, that "2 + 3 = 5" can be properly said to belong to the same "system" as, *e.g.* "2 + 2 = 4", and also can properly be said to belong to a different "system" from that to which "11 + 111 = 11111" and "11 + 11 = 1111" both belong, though I have no clear idea as to the sense in which these things can properly be said. Nor do I know whether Wittgenstein would have held, *e.g.* that in the case of *every* English word, it could not have the significance which it actually has in English unless it belonged to the same "system" as other English words, or whether he would have held that this is only true of *some* English words, *e.g.* of the words "five" and "four", and of the words "red" and "green".

But besides these two positive things, (α) and (β), which he seemed anxious to say about the meaning of words, he also insisted on three negative things, *i.e.* that three views which have sometimes been held are mistakes. The first of these mistakes was (γ) the view that the meaning of a word was some image which it calls up by association—a view to which he seemed to refer as the "causal" theory of meaning. He admitted that sometimes you cannot understand a word unless it calls up an image, but insisted that, even where this is the case, the image is just as much a "symbol" as the word is. The second mistake was (δ) the view that, where we can give an "ostensive" definition of a word, the object pointed at is the meaning of the word. Against this view, he said, for one thing, that, in such a case "the gesture of pointing together with the object pointed at can be

used *instead* of the word ", *i.e.* is itself something which has meaning and has the same meaning as the word has. In this connexion he also pointed out that you may point at a red book, either to show the meaning of " book " or to show the meaning of " red ", and that hence in " This is a book " and " This is the colour ' red ' ", " this " has quite a different meaning; and he emphasized that, in order to understand the ostensive definition " This is ' red ' ", the hearer must already understand what is meant by " colour ". And the third mistake was (ϵ) that a word is related to its meaning in the same way in which a proper name is related to the " bearer " of that name. He gave as a reason for holding that this is false that the bearer of a name can be ill or dead, whereas we cannot possibly say that the meaning of the name is ill or dead. He said more than once that the bearer of a name can be " substituted " for the name, whereas the meaning of a word can never be substituted for that word. He sometimes spoke of this third mistake as the view that words are " representative " of their meanings, and he held that in no case is a word " representative " of its meaning, although a proper name is " representative " of its bearer (if it has one). He added in one place : " The meaning of a word is no longer for us an object corresponding to it."

On the statement " Words, except in propositions, have no meaning " he said that this " is true or false, as you understand it " ; and immediately went on to add that, in what he called " language games ", single words " have meanings by themselves ", and that they may have meaning by themselves even in our ordinary language " if we have provided one ". In this connexion he said, in (II), that he had made a mistake (I think he meant in the *Tractatus*) in supposing that a proposition must be complex. He said the truth was that we can replace a proposition by a simple sign, but that the simple sign must be " part of a system ".

(*b*) About " propositions ", he said a great deal in many places as to answers which might be given to the question " What is a proposition ? "—a question which he said we do not understand clearly. But towards the end of (III) he had definitely reached the conclusion " It is more or less arbitrary what we call a ' proposition ' ", adding that " therefore Logic plays a part different from what I and Russell and Frege supposed it to play " ; and a little later he said that he could not give a general definition of " proposition " any more than of " game " : that he could only give examples, and that any line he could draw would be " arbitrary, in the sense that nobody would have

decided whether to call so and so a ' proposition ' or not ". But he added that we are quite right to use the word " game ", so long as we don't pretend to have drawn a definite outline.

In (II), however, he had said that the word " proposition ", " as generally understood ", includes both " what I call propositions ", also " hypotheses ", and also mathematical propositions; that the distinction between these three " kinds " is a " logical distinction ", and that therefore there must be some grammatical rules, in the case of each kind, which apply to that kind and not to the other two; but that the " truth-function " rules apply to all three, and that that is why they are all called " propositions ".

He went on to illustrate the difference between the first two kinds by saying that " There seems to me to be a man here " is of the first kind, whereas " There is a man here " is a " hypothesis "; and said that one rule which applies to the first and not to the second is that I can't say " There seems to me to seem to me to be a man here " whereas I can say " There seems to me to be a man here ". But, soon after, he said that the word " proposition " is used in *two* different ways, a wider and a narrower, meaning by the wider that in which it included all three of the kinds just distinguished, and by the narrower, apparently, that in which it included the first two kinds, but not the third. For propositions in this narrower sense he seemed later very often to use the expression " experiential propositions ", and accordingly I will use this expression to include propositions of both the first two kinds. The things which he had to say about experiential propositions, thus understood, were extremely different from those which he had to say about the third kind; and I will therefore treat these two subjects separately.

(α) Of experiential propositions he said in (I) that they could be " compared with reality " and either " agreed or disagreed with it ". He pointed out very early something which he expressed by saying " Much of language needs outside help ", giving as an example your use of a specimen of a colour in order to explain what colour you want a wall painted; but he immediately went on to say (using " language " in a different sense) that in such a case the specimen of a colour is " a part of your language ". He also pointed out (as in the *Tractatus*) that you can assert a proposition or give an order without using any words or symbols (in the ordinary sense of " symbol "). One of the most striking things about his use of the term " proposition " was that he apparently so used it that in giving an order you are necessarily expressing a " proposition ", although, of course, an order can

be neither true nor false, and can be " compared with reality " only in the different sense that you can look to see whether it is carried out or not.

About propositions, understood in this sense, he made a distinction in (II) between what he called " the sign " and what he called " the symbol ", saying that whatever was necessary to give a " sign " significance was a part of " the symbol ", so that where, for instance, the " sign " is a sentence, the " symbol " is something which contains both the sign and also everything which is necessary to give that sentence sense. He said that a " symbol ", thus understood, *is* a " proposition " and " cannot be nonsensical, though it can be either true or false ". He illustrated this by saying that if a man says " I am tired " his mouth is part of the symbol ; and said that any explanation of a sign " completes the symbol ".

Here, therefore, he seemed to be making a distinction between a proposition and a sentence, such that no sentence can be identical with any proposition, and that no proposition can be without sense. But I do not think that in his actual use of the term " proposition " he adhered to this distinction. He seemed to me sometimes so to use " proposition " that every significant sentence *was* a proposition, although, of course, a significant sentence does not contain everything which is necessary to give it significance. He said, for instance, that signs with different meanings must *be* different " symbols ". And very often he seemed to me to follow the example of Russell in the Introduction to *Principia Mathematica* in so using the word " proposition " that " propositions ", and not merely sentences, could be without sense ; as, for instance, when he said at the beginning of (II) that his object was to give us some " firm ground " such as " If a proposition has a meaning, its negation must have a meaning ". And, towards the end of (III), in connexion with the view at which he had then arrived that the words " proposition ", " language " and " sentence " are all " vague ", he expressly said that the answer to the question whether, when you say " A unicorn looks like this " and point at a picture of a unicorn, the picture is or is not a part of the proposition you are making, was " You can say which you please ". He was, therefore, now rejecting his earlier view that a proposition must contain everything which is necessary to make a sentence significant, and seemed to be implying that the use of " proposition " to mean the same as " sentence " was a perfectly correct one.

In connexion with the *Tractatus* statement that propositions, in the " narrower " sense with which we are now concerned, are

" pictures ", he said he had not at that time noticed that the word " picture " was vague ; but he still, even towards the end of (III), said that he thought it " useful to say ' A proposition is a picture *or something like one* ' " although in (II) he had said he was willing to admit that to call a proposition a " picture " was misleading ; that propositions are not pictures " in any ordinary sense " ; and that to say that they are, " merely stresses a certain aspect of the grammar of the word ' proposition '—merely stresses that our uses of the words ' proposition ' and ' picture ' follow similar rules ".

In connexion with this question of the similarity between experiential " propositions " and pictures, he frequently used the words " project " and " projection ". Having pointed out that it is paradoxical to say that the words " Leave the room " is a " picture " of what a boy does if he obeys the order, and having asserted that it is, in fact, *not* a " picture " of the boy's action " in any ordinary sense ", he nevertheless went on to say that it is " as much " a picture of the boy's action as " 2 + 3 " is of " 5 ", and that " 2 + 3 " really is a picture of " 5 " " *with reference to a particular system of projection* ", and that this system is " in no way inferior " to the system in which " 11 + 111 " is projected into " 11111 ", only that " the method of projection is rather queer ". He had said previously that the musical signs " ♯ " and " ♭ " are obviously not pictures of anything you do on the keyboard of a piano ; that they differ in this respect from what, *e.g.* " " would be, if you had the rule that the second crotchet is to stand for the white key on the piano that is next to the right of that for which the first crotchet stands, and similarly for the third and second crotchet ; but nevertheless, he said, " ♯ " and " ♭ " " work in exactly the same way " as these crotchets would work, and added that " almost all our words work as they do ". He explained this by saying that a " picture " must have been given by an explanation of how " ♯ " and " ♭ " are used, and that an explanation is always of the same kind as a definition, *viz.* " replacing one symbol by another ". He went on to say that when a man reads on a piano from a score, he is " led " or " guided " by the position of the crotchets, and that this means that he is " following a general rule ", and that this rule, though not " contained " in the score, nor in the result, nor in both together, must be " contained " in his intention. But he said, that though the rule is " contained " in the intention, the intention obviously does not " contain " any *expression* of the rule, any more than, when I read aloud, I am

conscious of the rules I follow in translating the printed signs into sounds. He said that what the piano-player does is " to see the rule in the score ", and that, even if he is playing automatically, he is still " guided by " the score, provided that he *would* use the general rule to judge whether he had made a mistake or not. He even said in one place that to say that a man is " guided " by the score " means " that he *would justify* what he played by reference to the score. He concluded by saying that, if he plays correctly, there is *a* " similarity " between what he does on the piano and the score, " though we usually confine ' similarity ' to projection according to certain rules only " ; and that in the same sense there is *a* " similarity " between automatic traffic-signals and the movements of traffic which are guided by them. Later on he said that for any sign whatever there *could* be a method of projection such that it made sense, but that when he said of any particular expression " That means nothing " or " is nonsense ", what he meant was " *With the common method of projection* that means nothing ", giving as an instance that when he called the sentence " It is due to human weakness that we can't write down all the cardinal numbers " " meaningless ", he meant that it is meaningless if the person who says it is using " due to human weakness " as in " It's due to human weakness that we can't write down a billion cardinal numbers ". Similarly, he said that surely Helmholtz must have been talking nonsense when he said that in happy moments he could imagine four-dimensional space, because *in the system he was using* those words make no sense, although " I threw the chalk into four-dimentional space " would make sense, if we were not using the words on the analogy of throwing from one room into another, but merely meant " It first disappeared and then appeared again ". He insisted more than once that we are apt to think that we are using a new system of projection which would give sense to our words, when in fact we are not using a new system at all : " any expression " he said " *may* make sense, but you may think you are using it with sense, when in fact you are not ".

One chief view about propositions to which he was opposed was a view which he expressed as the view that a proposition is a sort of " shadow " intermediate between the expression which we use in order to assert it and the fact (if any) which " verifies " it. He attributed this view to W. E. Johnson, and he said of it that it was an attempt to make a distinction between a proposition and a sentence. (We have seen that he himself had in (II) made a different attempt to do this.) He said that it regarded the supposed " shadow " as something " similar " to the fact

which verifies it, and in that way different from the expression which expresses it, which is not " similar " to the fact in question ; and he said that, even if there were such a " shadow " it would not " bring us any nearer to the fact ", since " it would be susceptible of different interpretations just as the expression is ". He said, " You can't give any picture which can't be misinterpreted " and " No interpolation between a sign and its fulfilment does away with a sign ". He added that the only description of an expectation " which is relevant for us " is " the expression of it ", and that " the expression of an expectation contains a description of the fact that would fulfil it ", pointing out that if I expect to *see a red patch* my expectation is fulfilled if and only if I do *see a red patch*, and saying that the words " see a red patch " have the same meaning in both expressions.

Near the beginning of (I) he made the famous statement, " The sense of a proposition is the way in which it is verified " ; but in (III) he said this only meant " You can determine the meaning of a proposition by asking how it is verified " and went on to say, " This is necessarily a mere rule of thumb, because ' verification ' means different things, and because in some cases the question ' How is that verified ? ' makes no sense ". He gave as an example of a case in which that question " makes no sense " the proposition " I've got tooth-ache ", of which he had already said that it makes no sense to ask for a verification of it—to ask " How do you know that you have ? " I think that he here meant what he said of " I've got tooth-ache " to apply to all those propositions which he had originally distinguished from " hypotheses " as " what I call propositions " ; although in (II) he had distinguished the latter from " hypotheses " by saying that they had " a definite verification or falsification ". It would seem, therefore, that in (III) he had arrived at the conclusion that what he had said in (II) was wrong, and that in the case of " what he called propositions ", so far from their having " a definite verification ", it was senseless to say that they had a verification at all. His " rule of thumb ", therefore, could only apply, if at all, to what he called " hypotheses " ; and he went on to say that, in many cases, it does not apply even to these, saying that statements in the newspapers could verify the " hypothesis " that Cambridge had won the boat race, and that yet these statements " only go a very little way towards explaining the meaning of ' boat-race ' " ; and that similarly " The pavement is wet " may verify the proposition " It has been raining ", and that yet " it gives very little of the grammar of ' It has been raining ' ". He went on to say " Verification determines the meaning of a proposition

only where it gives the grammar of the proposition in question " ; and in answer to the question " How far is giving a verification of a proposition a grammatical statement about it ? " he said that, whereas " When it rains the pavement gets wet " is not a grammatical statement at all, if we say " The fact that the pavement is wet is a *symptom* that it has been raining " this statement is " a matter of grammar ".

(*To be continued*)

II.—THE CONCEPT OF EVOLUTION

By T. A. Goudge

In this paper I wish to undertake a brief analysis of the concept of evolution. Nowadays this concept is widely used in such empirical sciences as physical cosmology, biochemistry, biology, psychology, sociology, etc., and it is therefore worth inquiring whether the concept has a precisely determinable meaning common to the different contexts in which it occurs. As a first step in that direction, I propose to discuss its meaning with special reference to biology, where its role is central and where it has received the most detailed consideration. It will thus be necessary to take account of certain broad conclusions reached by biologists about matters of fact. But my intention is to avoid making any pronouncements about such matters, and to limit myself to exploring the "logical geography" of the concept of evolution. That is to say, my interest in the concept is a philosophical one.

If we start with the ordinary non-technical use of the word "evolution", we can see that it is linked with the notion of change or process. It would sound self-contradictory to say: "The motor-car evolved from the horseless carriage but no changes of any kind took place"; and it would sound queer to say: "The number of primes below 100 has evolved". Yet there are many occasions on which we use the word "change" or "process" in a sense which excludes the word "evolution". Thus we say: "Water cooled to 0° C. changes to ice", but not: "Water cooled to 0° C. evolves into ice". Likewise, it is natural to describe a river-delta or a crystal in a super-saturated solution as "increasing" in size, but not as "evolving" in size. We might even speak of the river-delta or the crystal "growing", much as we say that an acorn grows to be an oak and that a child grows to be an adult. But we would not say that an acorn "evolves" into an oak or that a child "evolves" into an adult. Two centuries ago, indeed, it was the practice to apply the term "evolution" to the process which starts with a fertilized ovum and ends with a fully-formed offspring. Today the term "development" is reserved for this process whereby an individual is produced. Even at the level of ordinary speech, then, it appears that while evolution is a type of change, it is not identical with mere change of state, quantitative increase, individual growth or individual development.

Now there are three quite general questions we may ask regarding any type of change: *viz.*, What is it that changes? How does it change? and What causes it to change? Let us put these questions in terms of evolution and see what further light is shed by the answers contemporary biology gives to them.

What is it that evolves? Many biologists in the past would have said that living organisms, species of organisms, or simply species evolve. At present the view seems to be that this is an unsatisfactory way of talking because "living organisms" and "species" are high-level abstractions, some distance removed from the empirical data. What a field biologist observes are individual plants and animals, certain of which do or can interbreed, living successfully in a given geographical locality. These actually or potentially interbreeding individuals are said to form a "local population" or "interbreeding population". A group of populations, no matter how widely separated in space, will constitute a species—at least as far as bisexual animals are concerned—provided the members of the group are fertile with one another and are able to produce fully fertile offspring. The important point, however, is that a local population is basically what evolves. Such populations are therefore the ultimate evolutionary units.

Several consequences follow. (1) Since the associated individuals which make up a local population do not themselves evolve, there is a lower limit to evolution in the biological sense. Consequently, if "evolution" is used in connexion with the constituents of biological individuals, as it is when people talk about "biochemical evolution", the term must have a meaning other than its biological one. (2) Although biological individuals do not evolve, they obviously do change (develop, grow, degenerate, etc.). Hence the logic of the term "evolution" does not require that any reference be made to something immutable and permanent. (3) Every local population endures throughout a considerable stretch of time during which some of its constituent individuals die and others are born. Clearly, then, a population cannot be identified with the collection of associated individuals which belong to it at any particular moment of time. Yet there cannot be a population without the association of some individuals. (4) Every local population is amenable to investigation in quantitative terms, especially those employed by statistics. This is one of the most striking differences between the Darwinian and the modern outlook in biology. For Darwin the concept of evolution was predominantly qualitative. For contemporary biologists it is becoming

XUM

increasingly quantitative and associated with the statistical treatment of populations. To appreciate this aspect of the concept we must take up our other two questions.

How does evolution take place? A complete scientific answer to this question is not possible at present, even though an immense amount of evidence relevant to it has been assembled. The outline of an answer, however, is available; and since my concern is a philosophical one, the outline will be a sufficient basis for the broad issues I want to raise.

The individual organisms making up a local population at a given point of time are such that each of them has (*a*) an observable form, (*b*) observable functions, and (*c*) a determinate genetic constitution. If the population continues to exist, these organisms will be succeeded by others exhibiting slight differences from their parents with respect to (*a*), (*b*) and (*c*). Indeed, the total history of the population can be traced in terms of such differences among successive generations of its members. A comparison of an earlier with a later stage of the population's history would then lead us to say that the population had evolved. The rate at which it evolves is by no means always the same; nor is the occurrence of sudden large differences excluded, though it is apparently quite rare. But in general the evolution of a population takes place by the gradual, continuous change of the form, function and genetic equipment of its individual members through successive generations. This is the basis on which, after a very long period of time, a population may give rise to a new species if its members cease to be interfertile with the members of another population. Thus evolution is by no means synonymous with the multiplication of species, as Darwin seems to have supposed, and may in fact go on when no new species are being produced.

The statistical representation of this process is made possible because the genetical constitution of each individual organism is held to consist of a set of discrete units (genes) whose interaction controls all the observable characteristics of the individual. These units have a high degree of stability, and are transmitted in different combinations from parents to offspring. Each individual has a different set of genes from every other individual (save in the case of identical twins); but not every gene occurs in every individual of a population. By an act of abstraction it is possible to regard a population as a population of genes (or rather pairs of genes), and to assign to each gene a number signifying the frequency of its occurrence in the population. Since the laws governing the transmission of genes

from parents to offspring when all external influences are disregarded have been well-established, the behaviour of gene frequencies under a variety of natural conditions can be explored by employing statistical concepts. From this point of view the basic evolutionary process appears as a systematic change of gene frequency in a local population. This way of looking at evolution has some interesting philosophical consequences which I shall mention at a later stage.

The descriptive aspect of the concept of evolution includes not only the history of existing populations but also the history of all populations which have ever existed on the earth. As the description is pushed farther and farther back into the past it becomes more and more conjectural. Yet the evidence assembled strongly suggests that certain definite trends have taken place.

(i) There has been an absolute increase in the amount of living matter or in total metabolic processes on the earth. A greater number of individual organisms exist the longer the evolutionary process continues. (ii) There has been an absolute increase in the number of distinct species, from comparatively few in the remote past to the million or more known today. Evolution has been on the whole an expansive or radiating process, even though many species have disappeared from the earth in the course of time and occasional instances of evolutionary "convergence" have been established. (iii) Earlier organisms have generally been simpler in form and function than later ones, so that there has been an absolute increase in the internal complexity of the individuals belonging to populations. (iv) Since the first organisms were probably unicellular and hence small, an over-all increase in size must have occurred. This is only a general trend, to be sure, for in some phyletic lines descendants are demonstrably smaller than their ancestors. (v) Living things have exhibited a tendency to spread into every available environment. Whenever an environment has become able to support life, some population of organisms has almost always proceeded to occupy it, often becoming highly specialized as a consequence. This readiness to exploit the environment in every way possible has been called the "opportunism" of evolution, and is one of its most striking features.

Now when one thinks about the grand panorama of evolution in the light of these trends, one is tempted to ask at least two questions. The first of them may be put thus. From what source or sources have the continuously increasing variety and complexity of living organisms come? To this question, which

XUM

can be phrased in several ways, one of three answers has usually been offered. (1) At bottom, the whole course of evolution has been simply the history of the successive combinations into which the elementary physical constituents of the universe (electrons, protons, neutrons, etc.) have entered. Hence, the source of the increasing variety and complexity has been the reshuffling of these constituents so as to produce more and more diverse forms and more and more intricate aggregations. (2) The course of evolution has been a progressive actualizing of the potentialities of living matter. There has been an unpacking of an original complex which had latent within it all the variety and complexity which has subsequently appeared. (3) Evolution is neither a reshuffling nor an unpacking but a creating. The variety and complexity of organic forms have been creations of the evolutionary process, and require no reference to pre-existing physical constituents or organic potentialities. Those who subscribe to this position often say that there has been an "emergence of novelty", a "holistic creation", or "creative evolution".

The second question one feels tempted to ask is this. What is the source of the "opportunistic" feature of evolution, its tendency to spread into every available nook and cranny? In reply it has been said that we must regard this feature as the expression of a primordial life-force, *élan vital*, entelechy, etc., which is perpetually striving to exhibit itself in the maximum number of ways. The life-force has not allowed organisms to remain in just a few environments, but has driven them on into every possible environment even at the risk of excessive specialization and ultimate extinction. Sometimes it is said that the aim of the life-force was to produce man or perhaps super-man. But in any case, so the account runs, there is no other source than the life-force to which we can trace the undeniable opportunism of evolution.

At first blush the above questions seem quite legitimate and the answers to them at least relevant. On reflexion, however, certain doubts begin to arise. The answers, for example, are ostensibly statements of fact, yet it is hard to see what empirical evidence could enable us to determine their truth or falsity. There are no observations in terms of which they can be directly tested, and it is not obvious that they are indirectly testable (*e.g.* that any one of them can be shown to follow logically from directly verified statements). I am doubtful, therefore, whether the proposed answers are statements of fact at all. What about the questions? If we ask: "From what source did the Nile

delta come ? ", " From what source did this crystal derive its increased size ? ", " Where did Stonehenge come from ? ", etc., we can set certain empirical inquiries on foot and make certain observations in the light of which answers may be obtained. Such questions are certainly factual. But are there any empirical inquiries or observations to which we are led by asking about the source of evolutionary variety and complexity, or about the source of the tendency of organisms to spread ? It seems doubtful. These broad questions have the same verbal form as factual ones, but it does not follow that they are factual questions. They seem rather to be " armchair " attempts to get at the causes of evolution. But this is a factual not a speculative matter. We may, therefore, pass to a consideration of what is being said about it on empirical grounds.

What are the causes of evolution ? The most impressive conclusions here are those reached by biologists who have sought to combine the findings of genetics with the Darwinian doctrine of natural selection. The account they give may be outlined as follows.

In a given local population the genetic material present in individuals is such that they vary slightly from each other and from their offspring. Not only are the genetic units (genes and chromosomes) recombined in sexual reproduction so as to determine new characteristics of individuals, but at infrequent intervals one of these units will undergo a radical change or " mutation ". The effects of a mutation may be large, in which case they will generally prove lethal to the organism in which they occur ; or the effects may be small, in which case they may prove useful to the organism. The most important sort of mutation is that of a gene, for then a wholly new genetic unit comes into existence. It is sometimes said that these gene-mutations provide evolution with its " raw material ". All such changes are random in the sense that they are not elicited directly by the environment or by the needs of the individual organism at a particular time. But this does not mean that mutations do not have causes.

Now in a population some individuals of each generation die before they reproduce, usually because they fail to reach maturity. Their genetic constitutions have obviously no further influence on the population. Of those who do reproduce, some have more offspring than others. Hence the genetic constitutions of individuals with the most offspring will occur with the greatest frequency in subsequent generations, and will have the maximum determining effect on the population's

XUM

history. The expression "natural selection" is simply a convenient way of referring to this differential survival and differential reproduction in a population. Because of natural selection certain types of genetic constitution spread through a population in successive generations, while other types are gradually eliminated. If the environment is relatively stable, natural selection results in adaptation to a particular way of life. If the environment is changing rather rapidly, selection makes use of mutations to adapt organisms to hitherto unoccupied but available ways of life. Thus the causes of the evolution of a population are, according to this approach, the recombination of genetic units in reproduction, mutations and natural selection.

Even this rather bald summary is sufficient to show that some of the older ways of using the term "evolution" were misleading. For example, when it was said that evolution always involves a "struggle for existence", many people immediately thought of a hand-to-hand combat and of "nature red in tooth and claw". But we now see that "struggle for existence" is a highly misleading metaphor. For evolution is a process of differential perpetuation of certain types of genetic constitution in a group, and this does not necessarily involve any hand-to-hand combat or devouring of one individual by another. We may, if we like, describe the poppies growing in a field as "struggling for existence". But all this can mean is that some of the poppies die before reproducing and some manage to give rise to more offspring than others. Such a process is essential to evolution. Combat between individuals or the preying of individuals on one another is unessential. Combative and predatory activity often do occur, but they may prove to be counter-evolutionary influences if they occur within a species.

Another unfortunate way of talking about evolution is to say that it produces the "survival of the fittest". This phrase of Spencer's has frequently been criticized by people who failed to make clear what is wrong with it. If "the fittest" is a shorthand expression for "those members of a population which have the most progeny", and if "survival" is understood to refer to the spreading of certain types of genetic constitution in the population, then it is harmless, though unilluminating, to speak about "the survival of the fittest". If, however, this phrase is taken to have some other meaning, then its use is not warranted by biology. There are special dangers involved in applying it to individuals, since from a biological standpoint

an individual organism may be lacking in "fitness" as above defined (*i.e.* may be deficient in fecundity), and yet not be in the least degree frail or feeble. Conversely, it has been found that abnormal fecundity may co-exist with a deficiency in individual vigour. It seems best, therefore, to give up using the expression "survival of the fittest" since it is so misleading. Many biologists, indeed, prefer to talk not about "fitness" but about "adaptive value" in connexion with evolution.

The phrase "natural selection", like "struggle for existence", is metaphorical, as Darwin himself saw. Yet he often spoke as though the phrase designated a mysterious force which did the "selecting" by imposing itself upon a species *ab extra*. This way of speaking gave rise to grave misconceptions. His opponents interpreted him literally, and contended that it is absurd to attribute evolution to the play of "blind, mechanical forces" similar to those which produce earthquakes and hurricanes. But, of course, "natural selection" is not the name of a force acting on an evolving population from outside. It is, as we have seen, a way of designating a complex process going on *within* a population. The process is completely accessible to empirical observation, and has, moreover, both a negative and a positive aspect. The negative aspect is the gradual eliminating from the population of certain types of genetic constitution. The positive aspect is the maintaining of the existing level of adjustment of individuals carrying other types of genetic constitution, or the determining of new modes of adjustment to new environments by utilizing available mutations. This last feature is what leads biologists to speak of natural selection as "directing" or "orienting" the whole course of evolution.

If we accept the above causal analysis, then it seems proper to say that an evolving population is one which in interacting with the environment *changes itself*. Biological evolution is a kind of self-produced change. The causes of the change are the actions of individual constituents of the population, *i.e.* the recombination and mutation of individual genetic units together with the totality of actions giving rise to natural selection. The cumulative effect of these causes is the statistically determined shift in gene frequency in the population which constitutes the basic evolutionary process. Thus, evolutionary causality involves no non-individual factors; and all the individual factors are in principle observable. The results of the operation of the factors are also observable, but in populations not in individuals. Much of the alleged "mystery" attached to the evolutionary process disappears when it is seen in this light.

XUM

It may be urged, however, that if natural selection alone is what directs evolution, then the course which has been followed by life is a blind and accidental one. This consequence seems to many people utterly incredible. Without going into the various reasons they adduce for their incredulity, we may try to make out what is meant by using the terms " blind " and " accidental " in this context, and what appropriateness, if any, there is in such usage.

Suppose the postal authorities in a country find that over a given period the number of letters mailed every month without a stamp has steadily increased. This trend may be due to the combined activities of the country's letter-writers, each of whom mails his letters without reference to the similar acts of his fellows. Or, the trend may be due to a well-organized campaign on the part of letter-writers, so that each writer carries out his instructions to mail so many stampless letters every month. In the former case, we should describe the trend as " blind and accidental ", and in the latter case as " planned and purposive ". But note that when we use one of these pairs of adjectives, we imply that it makes sense to use the other pair under different circumstances involving the same individuals and the same acts. If this implication were not present, it would not make sense to use either pair. We describe a man as " blind " because under different circumstances it would make sense to describe him as " not blind ". But we do not describe a triangle or a cloud as " blind ", because there are no circumstances under which it would make sense to say that the triangle or the cloud is " not blind ".

Applying these considerations to the case of evolution, it would seem to follow that we can properly employ the adjectives " blind and accidental " only if, given the same individual constituents of populations, but different circumstances, it would make sense to employ the adjectives " planned and purposive ". The circumstances required are those in which the individuals act so as to realize a plan preconceived by them. But for the greater part of evolutionary history it was physically impossible, given the types of individuals which existed, for such circumstances to obtain. The devising and executing of plans depend upon the presence of physical and psychological equipment which organisms during this period not only did not, but *could not*, possess without becoming different organisms. To describe the course of evolution which resulted from their combined actions in accordance with natural selection as " blind and accidental ", seems to me as inappropriate as saying that a

triangle or a cloud is " blind ". Once organisms (*e.g.* human beings) appeared who are able to act collectively in the light of preconceived plans, it becomes appropriate to describe their history as " blind and accidental " or " planned and purposive ", depending on how they do in fact behave. Indeed, we may say that with the appearance of human beings, evolution becomes a process by which one population (mankind) not merely changes itself, but is able to direct the way in which it changes. When this capacity is exercised on a significant scale, human evolution will be properly described as a kind of self-directed change.

As long, then, as a biologist deals with the pre-human phase of evolution, his vocabulary need not include the term " purpose " (or its contradictory) in the sense in which that term signifies the execution of a preconceived plan. It may be, however, that the term is useful to describe in a metaphorical way the striving of individual organisms to live and reproduce, since the actions performed in this connexion are directed towards an " end ", though not one that is consciously sought. And it is the statistical consequences of the different degrees of success of the striving to live and reproduce, together with the statistical effects of known processes of heredity in populations of diverse structure, which appear to explain satisfactorily all the major phenomena of pre-human evolution so far discovered. Purpose enters here only in a metaphorical sense.

Many other aspects of this subject await discussion, such as the relations between the concept of evolution and the concepts of law, progress, irreversibility, etc. But these matters lie outside the limits of a short paper. Enough has perhaps been said, however, to show that the contemporary biological doctrine of evolution contains issues of great philosophical interest and importance to which wide attention should be directed.

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XUM

III.—HOW SPECIOUS IS THE 'SPECIOUS PRESENT'?

By C. W. K. Mundle

Mr. J. D. Mabbott concluded his paper 'Our Direct Experience of Time' (Mind, April 1951) by claiming that 'the specious present is "specious" in a sense not intended by its supporters'. His arguments may appear, at first sight, to have knocked the last nails in the coffin of a suspicious character. I suggest, however, that we should pause before we lower the coffin, for unless we are clear about the nature of the corpse its resurrection is inevitable. In section I of this paper, I try to show that Mabbott's criticisms of the 'specious present doctrine' miss their mark. In section II, I argue that one does not dispose of the doctrine by rejecting the metaphysical assumptions in terms of which it has been formulated by any particular philosopher. In section III, I distinguish several different ways in which the doctrine may be 'cashed' in terms of empirical features of our experience; and I claim that the doctrine, if cashed in one of these ways, must be accepted by those who adopt a common-sense theory of perception. I conclude, in section IV, by discussing the (termino-)logical issues which arise when we ask whether the term 'specious present' is appropriate as a description of the doctrine in question. My topic may appear to be very restricted, but the issues raised are, I think, far-reaching; for all of the problems involved in philosophical theories about time are implicit in the question as to how we should analyse and describe what has been called the 'specious present'.

I

My main complaints about Mabbott's procedure are: (i) that he does not acknowledge the motive of philosophically-minded supporters of the specious present doctrine; and (ii) that he takes a most peculiar interpretation as being *the* correct analysis of the doctrine.

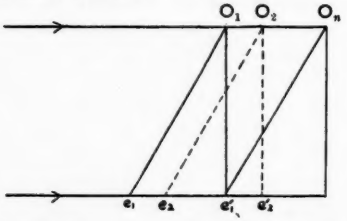
The classical problem with which Mabbott opens his discussion is formulated 'Has an idea duration?' He proceeds to discuss, as being the modern formulation of the same problem, the question 'How long does a sense-datum last?' Now when Mabbott says of the specious present doctrine 'The aim was to discover the unit of temporal experience' (p. 162), he seems to be

assuming that the philosophers who invoke this doctrine do so because they think of a person's experiences as consisting of a row of sense-data laid out, so to speak, end to end, like a row of bricks in a wall, and are concerned to measure the length of the individual sense-datum. That Mabbott is making such an assumption is indicated, for example, by his saying 'it is possible to be using two senses at once (at a ballet performance). But then I shall be simultaneously apprehending specious presents of different lengths. And what then is *the* unit?' (p. 163).

Admittedly some philosophers have spoken of 'sense-data' in ways which seem to expose them to the question 'How long does a sense-datum last?' But the interest of philosophers in the specious present doctrine does not depend upon their accepting what has been dubbed 'brick and mortar psychology'. The source of James's interest in the doctrine is made plain by the questions with which he introduced the discussion (*Principles of Psychology*, Vol. I, ch. XV), *e.g.*, 'What is the *original* of our experience of pastness, from whence we get the meaning of the term?' (p. 605), and by the ways in which he formulated his answer, *e.g.*, 'the original paragon and prototype of all conceived times is the specious present, the short duration of which we are immediately and incessantly sensible' (p. 631). James did not distinguish questions of genetic psychology, concerning the source or origin of temporal concepts, from logical questions concerning their meaning. Philosophers like Broad and Russell have been primarily concerned with the latter. Their main concern in appealing to the 'specious present' is to claim that we are directly acquainted with instances of certain temporal characteristics (*e.g.*, the relations of temporal precedence and/or overlapping), and that all temporal words or concepts can be defined in terms of these. To many philosophers this claim may appear too obviously true to merit discussion, but Mabbott's thesis presents them with a challenge. If we accept Mabbott's view that the specious present doctrine is an irretrievable muddle, it looks as if we might be forced to describe temporal concepts as *a priori*. When Broad wrote *The Mind and its Place in Nature*, he had apparently lost faith in the specious present doctrine and was, as a result, doubtful whether the term 'past' could be ostensively defined. He recommends, as the only alternative, the view that pastness is a 'categorial' (as opposed to 'empirical') characteristic (p. 265). We ought therefore to consider whether the use made of the specious present doctrine by empiricists is legitimate.

XUM

My second complaint against Mabbott is that he seriously misrepresents the account of the specious present offered by Broad in *Scientific Thought*. The diagram used by Broad (p. 349) is reproduced below.



Broad interprets the diagram in the following way. The lower horizontal line *ee* represents a slice of the history of an observer's 'sensible-fields', position on the line representing date, the direction left-to-right representing earlier-to-later. The upper horizontal line OO represents a slice of the history of the observer's acts of sensing. The apex of a triangle represents a 'momentary act of sensing'. The length of the base of the triangle represents the duration of the 'sensible event' grasped by (or the content of) this act of sensing. This duration, which Broad treats as having a constant value, τ, is the duration of the observer's specious present.

It is not clear to me why Mabbott concludes (p. 161) that Broad's account 'depends on momentary acts of apprehension'. Broad writes 'I shall begin by assuming literally momentary acts of sensing and shall then correct this abstraction' (p. 348), and he fulfils this promise without delay. He indicates (p. 350) that we are to imagine a *continuous* series of momentary acts between O_1 and O_2, and that 'momentary act' is to be defined as a limit by the method of Extensive Abstraction; and he says (p. 351) that 'In real life we may assume that our acts of sensing are not momentary, but are processes that last for a finite time'. Thus when Mabbott appropriately suggests (p. 161) that we interpret Broad's diagram by analogy with a searchlight which moves along the upper line and illuminates a constant span of the lower line, there was no need to say (as if Broad would have disagreed with this feature of the analogy) 'When a searchlight moves over a landscape it does not illuminate one object *repeatedly*; it illuminates it *continuously*'.

Mabbott's most serious misunderstanding is, however, in interpreting Broad as accepting the theorem that the duration of the specious present of an act of awareness varies inversely

with the duration of the act. This would entail, for example, that the longer one looks at or listens to something, the briefer is the duration of what is seen or heard! This theorem would be wildly paradoxical, and in fact Broad says no such thing. I shall try to elucidate the passage which is the source of the misunderstanding.

Broad is considering sensible events, like $e_2\,e'_1$, whose duration is less than that of the specious present (τ), and he is asking what is the maximum duration of an act of sensing *throughout* which the *whole* of such an event is sensed. Obviously $O_1\,O_2$ is the longest act throughout which the whole of $e_2\,e_1'$ could be sensed, for the earliest part of $e_2\,e'_1$ will not be sensed by any act which occurs after O_2, and the latest part of $e_2\,e'_1$ will not be sensed by any act which occurs before O_1. Broad's argument is perhaps most readily grasped if we picture the triangle as the beam of a moving searchlight and picture $e_2\,e'_1$ as, say, a white sheet. Broad's original question becomes—throughout what period will the whole sheet be illuminated? Now obviously this period will vary if we change the width of the sheet. If the sheet were of exactly the same width as the base of the beam, the whole of it would be lit up only for an instant; the narrower the sheet the longer the period during which the whole of it would be illuminated. Alternatively, we may consider variations in the distance through which the searchlight moves. The shorter the distance traversed by the searchlight, the wider the sheet, the whole of which could be illuminated throughout this movement (= the briefer an act of sensing the greater the duration of the event, the whole of which can be sensed throughout this act). Hence the theorem which Broad very carelessly formulated by saying 'In general, we notice that the shorter the time-lapse between two of O's acts of sensing, the longer is the event which is sensed in both of them; and that, as the lapse tends to nothing, the duration of the event tends to τ' (p. 349). Broad ought to have reformulated this theorem in the ensuing passage in which he proceeds to 'remove the supposition of literally momentary acts'. It seems clear, however, that what Broad meant to do was to draw attention to the following implication of his model:—

if E is any sensible event whose duration is less than τ,
and if x is the duration of E,
and if y is the duration of the longest act throughout which the whole of E can be sensed,
then $x + y = \tau$, so that x and y vary inversely.

Since Broad thought that this implication of his model was of some importance, he ought to have formulated it much more clearly than he did. Broad never maintains, however, what Mabbott takes him to mean, namely that the *specious present* of act $O_1 O_2$ is the *duration* of $e_2 e'_1$, and that, in general, 'the *specious present* of an act of awareness varies inversely with the duration of the act' (pp. 158-159). Broad nowhere uses such language, and it would have been glaringly inconsistent for him to do so. The theorem obscurely formulated by Broad is inferred by assuming that the duration of O's specious present is constant, whereas, in Mabbott's interpretation, what is inferred is that this duration is variable. Mabbott has, in effect, identified the duration of the specious present with y instead of τ. It transpires that the interpretation of the specious present which Mabbott attacks is not Broad's. Indeed it seems unlikely that anyone had hitherto entertained the paradoxical notion that the duration of an act of sensing and the duration of its content vary inversely and never overlap. I shall not discuss the criticisms of Broad which Mabbott makes in section II of his paper, since these are directed against his man of straw, and in particular against the view that the specious present of act $O_1 O_2$ is (the duration of) $e_2 e'_1$.

II

Broad adopted what may be called an 'act-object' interpretation of experience, *i.e.*, he assumed that any conscious state involves both acts of sensing and their objects, variously described as 'sensa', 'sensible events', 'sensible fields', etc. James adopted the same view in the *Principles*, though he spoke of 'pulses of thought' or 'passing thoughts' in place of 'acts of sensing'. In their accounts of the specious present, the basic metaphysical assumption is that acts of sensing are datable events or processes. Given this assumption, one of the main problems posed by the specious present doctrine is to decide what temporal relation holds between an act of sensing and its content. Broad chose a right-angled triangle to represent a 'momentary specious present', in order to indicate that the date of a momentary act coincides with that of the later boundary of its content. James, on the other hand, spoke of the specious present having 'a vaguely vanishing backward and forward fringe' (p. 613), which suggests that he considered that part of the content of an act of sensing is later than the act, and would have depicted a momentary specious present by an isosceles triangle.

Mabbott wants to describe the temporal relation between an act of sensing and its content differently from either James or Broad. Mabbott writes 'Suppose I listen to a drum-tap group and then listen to a second drum-tap group and identify it without error as having the same number of taps as the first. The question is this. How long did the act of awareness take which apprehended either group? Surely the most natural answer is that the act took as long as the group' (p. 164). Mabbott made this point as a criticism of Broad's model. The latter could, however, be defended by pointing out that Mabbott's question is ambiguous. 'The duration of the act which apprehends the drum-tap group' could be interpreted, in terms of Broad's diagram, to mean several different things, including (*a*) the difference between the time when the last tap enters the specious present and the time when the first tap leaves the specious present. In that case, the duration of this act is τ minus x, where x is the duration of the group; or (*b*) the difference between the time when the first tap enters the specious present, and the time when the last tap enters it. Then the duration in question would be x. Mabbott interprets his question in sense (*a*), but a person who adopted Broad's model could interpret it in sense (*b*), and thereby conform to what Mabbot considers the most natural convention. Thus Mabbott's point does not dispose of Broad's model, and Mabbott does not raise the more fundamental questions as to whether it makes sense to postulate datable acts of sensing, and to speak of a temporal relation holding between such an act and its content.

I suggest that Broad's basic assumptions are undermined by the following considerations. Why, we may ask, did Broad choose a triangle to represent a 'momentary specious present'? This choice was, I suspect, made from two motives:—

(i) to specify the relative dates of acts and their contents, and (ii) to symbolise a many-one relation between sense-contents, occurring at different times (say, the several notes in a musical phrase), and an entity by which these are 'combined' or to which these are 'co-presented'. It is tempting, and many philosophers have thought it necessary to invoke an entity to perform 'synthesis' or 'syngnosis' or 'integration', in order to explain the fact that sense-contents occurring at different times are perceived as a gestalt, *e.g.*, as a melody or as a thing changing.

It seems to me, however, that these motives are incompatible. If the fact that sense-contents occurring at different times are

perceived as a gestalt implies that they are combined by a further entity, surely we cannot treat this entity as datable without being involved in a vicious infinite regress. This may be overlooked if we confine our attention to the concept of a momentary act of sensing. But a process of watching or listening (which would have to be represented in Broad's diagram by a segment of OO) contains phases which occur at different times. The question as to how these phases are combined to yield awareness of a gestalt would surely be just as relevant (or irrelevant) as the original question as to how sense-contents are combined. If Broad met this argument by saying that he does not attribute to acts of sensing the function of synthesis or syngnosis, one may well ask what *other* reason could be offered for postulating such entities. Surely no one claims to be able, by introspection, to discriminate an act of sensing from its content. I conclude then that 'acts of sensing' (or James's 'pulses of thought') do not fulfil the purpose for which they were introduced, *i.e.*, to provide empirically respectable substitutes for mind-substances. If any philosopher is to be so old-fashioned as to entertain the notion of immaterial entities, let the 'ghosts' be substantival!

However, to reject the assumption of acts of sensing, *qua* datable events or processes, is not to dispose of the specious present doctrine, for this may be interpreted in terms of alternative metaphysical theories. I can see no reason why this doctrine should be rejected by philosophers who hold that consciousness is attributable to an entity to which temporal concepts do not apply—a 'transcendental self' or 'pure ego'. T. H. Green was surely accepting this doctrine in fact, if not by name, when he wrote 'Every object we perceive . . . requires in order to its presentation the action of a principle of consciousness, not itself subject to conditions of time, upon successive appearances, such action as may hold the appearances together, without fusion, in an apprehended fact' (*Prolegomena to Ethics*, para. 64). To represent spatially such an interpretation of the specious present, the least inappropriate model might be to picture the triangle (the beam of the searchlight) as stationary, and the contents of the base line as moving continuously to the left ('receding into the past'). Some features of such a model would be irrelevant. Since the apex of the triangle would represent an entity to which temporal concepts like 'date' do not apply, it would be irrelevant whether one chose a right-angled or an isosceles triangle. Again, one would provoke irrelevant criticism if one showed the base line as extending to

the right of the triangle. There may be some grounds for supposing that future events are, so to speak, laid out ready for inspection, namely the evidence for 'precognition'; but the specious present doctrine in no way depends on this supposition. One can, however, think of events as coming into existence (as 'becoming' in Broad's sense) at the later boundary of the specious present, and modify one's diagram accordingly.

If, on the other hand, it is deemed superfluous to invoke an entity to perform synthesis or syngnosis, the specious present doctrine could still be formulated in terms of the metaphysical theory, often called 'Neutral Monism', according to which all other alleged entities are logical constructions from sense-data. An interpretation acceptable in principle to philosophers of this school was suggested by Broad in his *Examination of McTaggart's Philosophy* (Vol. II, Part I, pp. 281-288). The diagram which Broad used there bears a general similarity to the one he used in *Scientific Thought*, and perhaps it was this that led Mabbott to assume that Broad was merely repeating and elaborating his earlier account of the specious present. However, in the diagram used in the later work Broad omits the line OO, and, in his exposition, he drops all talk about 'acts of sensing'. The right-angled triangle is now given quite a different significance. The apex of the triangle represents not, as before, an act of sensing, but merely the maximum degree of a characteristic which Broad calls 'presentedness'; and the height of the different parts of the sloping side of the triangle, which previously had no significance, represents the degree of presentedness belonging the different parts of the content of a momentary specious present. Broad does not explain what he means by 'presentedness' beyond saying 'This is meant to denote a psychological characteristic'. Mabbott suggests that this is Broad's name for what Hume called 'force and vivacity'. Provided that 'presentedness' is taken to refer to an empirical characteristic of sense-data, the model suggested here by Broad should be acceptable in principle to Hume or his followers.

(I am not, of course, suggesting that Broad espoused Neutral Monism when he wrote his work on McTaggart! Broad was, however, attempting to confine himself to phenomenological description when he gave this later account of the specious present. It is included in a section labelled 'Independent Account of the Phenomenology of Time'. Mabbott might argue that when, in this later account, Broad spoke of objects being 'prehended', he was covertly retaining the concept of acts of sensing. This is probably true. All I am saying is that taken

XUM

at its face value, Broad's later account is acceptable in principle to defenders of Neutral Monism—for they could interpret 'prehended' as 'having some degree of presentedness'.)

The specious present doctrine might also be interpreted in a way acceptable in principle to a physicalist. A physicalist who maintained that his sensations were to be identified with disturbances in his brain might use the diagram given by Broad in *McTaggart's Philosophy*. He could take the height of the sloping side of the right-angled triangle to represent not a 'psychological' (introspectable) characteristic, but the amplitude of electrical disturbances produced in the brain by stimulation of the sense-organs. Such disturbances are believed to die down gradually. Alternatively a philosopher who adopts a common-sense ('Naive Realist') theory of perception could use the sort of diagram which I suggested to illustrate the 'pure ego' interpretation; letting the apex of the triangle represent oneself *qua* organism and the base line represent states of (or events or phases in the history of) external material objects.

There are then a variety of ways of interpreting the specious present doctrine according to the metaphysical assumptions adopted. This doctrine cannot, I think, be disposed of on *a priori* grounds, *unless* it can be shown that any attempt to think of temporal characteristics by means of spatial models or metaphors is a logical misdemeanour. That this is so has recently been argued by Professor J. J. C. Smart,[1] who warns us against 'spatialising time', which he describes as a case of 'shifted syntax'. I shall confine myself to making three comments on Smart's thesis.

(i) As Smart acknowledges, the spatial metaphors that we use in talking and thinking about time seem inescapable, even when we recognise them as metaphors. We are not, of course, obliged to think about time, but if we do this, our choice, it seems, is not *whether* we shall use spatial metaphors but *which* we shall use. If that is so, there is a case for making clear and explicit the various spatial models in terms of which we may interpret vague expressions like 'the flow of time', for only then can one decide in what respects one's model is appropriate, in what respects misleading.

(ii) We must admit that, in some respects, spatial models are liable to mislead. For example, any model in which the 'flow of time' (the transitory feature of experience) is depicted as a movement is liable to prompt questions concerning the speed of this movement (is it constant? etc.). To accept such questions

[1] 'The River of Time', MIND, October 1949.

as meaningful, as J. W. Dunne did, is to commit oneself to a second time-dimension by reference to which the speed of this movement is to be measured—and so *ad infinitum*. But surely we *need* not be misled into asking or trying to answer such questions.

(iii) Smart may be right in considering it a logical misdemeanour to 'spatialise time', but I do not find his diagnosis either clear or plausible. The crux of his argument is stated: 'Substances exist in space; they are related to one another in a 3-dimensional order. Events are in time; they are related to one another in an order of earlier and later. Now if we think of events as changing, namely in respect of pastness, presentness and futurity, we think of them as substances changing in a certain way. But if we substantialise events, we must, to preserve some semblance of consistency, spatialise time' (p. 493).

I suggest, however, that our propensity to spatialise time is not due primarily, if at all, to a tendency to 'substantialise events' *qua* thinking of them as things which change in respect of temporal predicates. The mere fact that we think of material objects as having histories, as changing in respect of non-temporal characteristics, is sufficient to account for this propensity. Smart's segregation of substances existing in space and events existing in time seems to bristle with difficulties. I can understand why Smart should consider that events do not exist in space, for he uses 'event' in a very restricted sense to refer to an 'actualisation' as distinct from a process, *e.g.*, to the winning of a race and *not* to the running (pp. 487-490). But I find it difficult to understand why Smart should deny (and his argument seems to depend on doing so) that substances 'exist in time'. Substances, he says, 'are related to one another in a 3-dimensional order'. Presumably by 'substances' Smart means such things as cricket-balls, mushrooms and persons. But any statement about the spatial relations between such things (*e.g.*, between a cricket bat and ball) is incomplete unless it specifies or indicates the date or period in question. Smart's thesis, I take it, is that our propensity to represent temporal relations spatially is due to the fact that our language contains event-expressions about whose logic we get muddled. This view can be countered by considering the possibility of a language in which there were no event-expressions in Smart's sense. It seems to me obvious that the user of such a language would be no less inclined than we are to represent spatially the relations of temporal precedence and overlapping (*a*) between

XUM

different events *qua* processes (*e.g.*, a flash and a roar), (*b*) between different states of a substance (*e.g.*, the traffic light being red, and its being green, and (*c*) between substances which exist at different times (*e.g.*, my grandfather and myself).

III

In this section I shall consider whether, and if so how, we can 'cash' the specious present doctrine by reference to empirical features of our experience. I shall not debate the merits of the rival metaphysical theories in terms of which the doctrine may be conceived. I shall now mean by 'the specious present doctrine' what is common to the different interpretations distinguished in the last section. It is not easy to formulate this common element in a way which would be acceptable to all parties. Perhaps the best formula for this would be to say that the contents of one's immediate experience have a duration, leaving it an open question whether the contents in question are to be described as 'sensible events', 'sensa', etc., or in material object language. In the latter case, the specious present doctrine could be expressed as the view that a short slice of the history of material objects is immediately experienced. Notice, however, that we cannot here replace 'immediately experienced' by 'perceived' or 'seen' without reducing the doctrine to a tautology. It is beyond question that we perceive things as enduring and changing, that we speak of 'seeing' a thing move from A to B. The specious present doctrine, in our present metaphysically neutral sense, is a mere platitude, *unless* we draw a distinction between what is perceived (or 'perceptually accepted' or 'judged') and what is sensed (or 'sensibly presented' or 'immediately experienced'). This last distinction, however we may choose to express it, seems to me essential in discussing the phenomenology of perception. We perceive a material thing as a solid, when what is sensibly presented is, at most, its surfaces, usually only its front surface. We perceive a thing as enduring, but is a 'slice' of its duration sensibly presented? Whether this sort of question seems to one pointless or pertinent will depend, I suppose, on whether one is interested in the phenomenology of the transitory aspect of experience.

The specious present doctrine *might* of course be cashed without adopting a phenomenological standpoint, might be cashed in terms of the results of psychologists' experiments.

But in that case it seems arbitrary which set of experiments we should choose. James assumed that Wundt and his followers had been measuring the duration of the specious present in experiments designed to measure the duration of the longest group of sounds which a person could correctly identify, without counting its members. On this basis he took the duration of the specious present to be about 12 seconds (*Principles*, p. 613). Mabbott questioned the relevance of this procedure, saying 'I think the truth is that they discovered the group [which] could be identified without error, and inferred that it was therefore remembered as a whole and must accordingly have been heard as a whole' (p. 156). James could, however, equally well have cashed the doctrine in terms of a different set of experiments, *e.g.*, those designed to ascertain the duration between two clicks which a person estimates most accurately. This was found by several of James's contemporaries to be about 0·75 second. Incidentally, Mabbott suggests (p. 157) that James took the results of *both* of these types of experiment as giving a measure of the specious present, and comments 'It does not seem to have troubled people at the time that the two sets of experiments reached such very different results'. I can, however, find no confirmation of Mabbott's interpretation in what James says in the *Principles*, though even if Mabbott were correct, the apparent inconsistency might be resolved by taking the former experiments to measure the maximum and the latter to measure the average duration of the specious present.

However, the use made by philosophers of the specious present doctrine is not discredited by questioning the relevance or the consistency of such experimental data, for philosophers who invoke this doctrine do not usually make any appeal to such evidence. Sometimes they speak as if the doctrine were self-evident, *e.g.*, Professor Ayer says baldly and without any argument 'I take it that instances of the relation of temporal precedence are sensibly given in our experience' (Presidential Address to the *Aristotelian Society*, 1951, p. x). Sometimes they ask us to reflect on what is implied by the fact that we see things moving or changing. An illustration used by both Broad [1] and Russell [2] is the fact that one sees the movement of the second-hand of a watch in a way that one does not see the movement of the hour-hand. We are presumably meant to infer from this fact that since movement cannot be predicated of an instantaneous state of a physical object, we must be acquainted

[1] *Scientific Thought*, p. 351. [2] *Human Knowledge*, p. 226.

XUM

with a brief slice of the history of the second-hand. This argument has, however, been challenged by Professor Paton,[1] who wrote 'it seems to me that if in a moment I can sense several different positions of a second-hand, then these different positions would be sensed as being all at the same moment. That is to say what I should sense would not be a movement, but a stationary fan covering a certain area, and perhaps getting gradually brighter towards one end.'

This objection may at first sight appear formidable. Paton's point, I take it, is that if the specious present doctrine were true, our experience when we look at a watch ought to be different from what it in fact is; for what could be meant by the doctrine, unless it meant, in the case in question, that when the second-hand has visibly reached one position, it is still being sensed (though perhaps less vividly) as occupying its recent positions? In that case, what we see should look blurred in a way in which the second-hand does not look blurred. I suggest that this objection can, however, be met by pointing out that the feature, which Paton failed to find when he looked at the second-hand, is found when one looks at things which move (traverse a given optical angle) more quickly. A simple experiment will disclose this feature. Hold a small piece of white paper at arm's length against a dark background, and move it across (or down) through a distance of about a foot. The speed of movement need not be faster than would prevent one from 'following' the paper by eye-movements. But instead of doing this, gaze at a point in the middle of the paper's path (call it 'place 1'). When I do this, I see, for a brief but appreciable period, a white streak, and this streak does not disappear until *after* the paper is seen, out of the corner of my eye, to have halted (at what I shall call 'place 2'). This indicates that, as we may express it provisionally, visual sensations linger and fade for an appreciable period. (I estimate this period as being of the order of 0·1 to 0·2 second.) This fact is not readily noticed because one follows a moving object in which one is interested by head- and eye-movements, and does not attend to the consequent blurring of the background. This feature of our visual experience provides *one* way in which we can cash the specious present doctrine. One of the illustrations which Russell uses in *Human Knowledge* suggests this way of cashing the doctrine. After using the watch illustration, he writes 'When I see a rapid movement, such as that of a falling star . . . I am aware that

[1] *In Defence of Reason*, pp. 106-107. This appears in the paper on 'Self-Identity', reprinted from MIND, July 1929.

one part of the movement is earlier than another, in spite of the whole being comprised within one specious present; if I were not aware of this, I should not know whether the movement had been from A to B or from B to A, or even that change had occurred ' (p. 226).

The fact, which we have described by saying that visual sensations linger and fade, *enables* us to cash the specious present doctrine, but does it, we may ask, *oblige* us to accept this doctrine ? Does it oblige us to agree with Ayer that ' instances of the relation of temporal precedence are sensibly given ' ? It seems to me that this is so if, but only if, we adopt the common-sense theory of perception. I shall try to explain briefly my reasons for saying this. Consider again our simple experiment with the piece of paper. I found that there is a brief but appreciable period during which I see the paper stationary at place 2, and still see a white streak at place 1. On the common-sense theory of perception, what I am seeing is literally a surface of an external physical object, and such an object cannot occupy different regions of space at the same time; so I shall have to describe my experience by saying that I am still seeing *the paper* at place 1 when it has visibly reached place 2; *i.e.*, seeing the last phase of the streak at place 1 must be interpreted as seeing the paper in a place it occupied a moment ago, or seeing a recent event—the paper passing through place 1. Thus if the common-sense theory of perception is accepted, the facts in question imply that we see a short slice of the history of material objects. The only grounds on which this conclusion could, I think, be challenged are (i) if it turns out that other people's experiences, when performing the experiment in question, are not like mine. (My visual experience *may* be abnormal in this respect, but I suspect not, as I have asked several others to try this experiment and they have concurred in my way of describing what is seen); or (ii) if a philosophical exponent of common-sense argued that such things as streaks are not to be identified with surfaces of material objects, though non-streaky and unblurred patches of colour are to be so indentified. But this would obviously be a *very* difficult position to defend, for being streaky or blurred is a matter of degree. If one waves a cigarette before one's eyes, starting slowly but gradually accelerating the speed, at what point would one cease to see the surface of the cigarette ? And what would be the ontological status of that which one then starts to see ?

I shall assume then that we must accept one version of the specious present doctrine, if we hold a common-sense theory of

perception. But the argument we have used is no longer compelling if we adopt a sensum theory (terminology). For in that case, it seems quite arbitrary whether one describes one's experience in performing our experiment by saying (i) that one senses a sensum which *was* at place 1 about $^1/_{10}{}^{th}$ of a second earlier, or (ii) that one senses the contemporary *momentary* state of a sensum at place 1. The point may be expressed differently by saying that my earlier description—'that visual sensations linger and fade'—is ambiguous. On the common-sense theory, the lingering and fading must be attributed to the seeing and not to the object seen, for a physical object (at any rate as conceived by the plain man) cannot be lingering in place 1 when it has visibly reached place 2. But if we adopt a sensum theory, the lingering and fading could be regarded as objective characteristics of sensa, if sensa were conceived as 4-dimensional entities; and the fact that sensa were *conceived* (or 'sensa' *defined*) as 4-dimensional entities would be compatible with maintaining that only instantaneous 'cross-sections' of the history of a sensum are sense-given, compatible, that is, with rejecting the specious present doctrine. I do not profess to know which of the alternative conventions, (i) or (ii), would be chosen by the majority of the philosophers who use a sensum terminology. Apart from Broad, such philosophers have not paid much attention to the phenomenology of time-perception.

The method of cashing the specious present doctrine discussed above has been illustrated only in the case of vision. *Prima facie*, the terms 'fading' and 'lingering' are more applicable to other sensations, *e.g.*, tickles and smells, than to visual sensations. However, even if we adopt the common-sense theory of perception, our other senses do not provide the unambiguous case for cashing the specious present doctrine that is provided by vision. For example, if I stroke my cheek with a cold penny, the so-called 'after-sensation' lingers for several seconds; but in this case, my cheek is available throughout the experience (and in the appropriate place!) as that to which the sensation can be referred; so it seems quite arbitrary whether I speak of my being fadingly aware of a recent state of my cheek, or of being simultaneously aware of the momentary state of my cheek. If, however, our interest in the specious present doctrine is solely to provide a way of giving ostensive definitions of temporal concepts, it is immaterial whether the doctrine is cashable in terms of all sensations, so long as it is cashable in some cases.

There is another way in which we may cash the specious

present doctrine. Consider what Russell says in *Analysis of Mind* (p. 174)—'Everyone knows the experience of noticing (say) that the clock *has been* striking, when we did not notice it while it was striking. And when we hear a remark spoken, we are conscious of the earlier words while the later ones are being uttered, and this retention feels different from recollection of something definitely past. A sensation fades gradually, passing by continuous gradations to the status of an image. This retention of the immediate past in a condition intermediate between sensation and image may be called "immediate memory". Everything belonging to it is included with sensation in what is called the "specious present".' It seems to me that Russell's appeal to 'immediate memory' provides a way of cashing the specious present doctrine which is quite distinct from the one I have discussed above, and that Russell was blurring their difference.

A salient feature of what Russell calls 'immediate memory' is that in such cases one seems to have direct access to events which are no longer 'going on', *e.g.*, to sounds which have audibly terminated. Consider the difference between (*a*) attending to a sound which is audibly going on, or a streak which is visibly there; and (*b*) attending to a sound just after it has audibly terminated, or a streak just after it has disappeared. The difference is surely obvious when we consider it, though not obvious in the sense that we are accustomed to making the distinction. Let us mark this distinction by saying that (*a*) is attention to something 'sensibly presented', and (*b*) is attention to something 'immediately remembered'. Our first method of cashing the specious present doctrine was by reference to attention of kind (*a*), attention to a sensibly presented white streak. A second method of cashing the doctrine is by reference to attention to what is not (in this sense) sensibly presented. Russell seems to overlook this point, and to blur the difference between the two kinds of attention when he writes 'A sensation fades gradually, passing by continuous gradations to the status of an image'. This statement seems appropriate to some sensations but not to others; appropriate to sounds, like the reverberations of Big Ben, which tail off gradually, so that there comes a stage when one cannot be certain whether one has ceased to hear them, or is imagining that one hears them or is remembering them; but inappropriate to sounds whose termination is, as we would say, 'abrupt' or 'sharp' or 'clear-cut', *e.g.*, each of the pips of the Greenwich time-signal. If so-called 'immediate memory' occurred only in cases when one

XUM

was experiencing sensations of the former kind, sensations whose termination is not abrupt, one would not be entitled to draw a sharp distinction between the two methods of cashing the specious present doctrine. Consider Russell's example—'noticing that the clock *has been* striking, when we did not notice it while it was striking'. In such a case, the 'noticing' could be attributed to one's hearing a noise which was still going on—caused by the continuing vibration of the chiming mechanism of the clock—plus an inference, conscious or unconscious. But in fact one seems to be able to 'inspect' sounds which have audibly terminated.

I live near a shunting yard, and sometimes when falling asleep I am disturbed by a short series of 'sharp' blasts from a railway engine, and I have found myself attending to such sounds after and only after they have audibly terminated. The sounds still seem 'immediately accessible', in the sense that I can observe their characteristics, *e.g.*, can count them and discriminate their relative durations. By all subjective criteria this kind of observation is as 'intuitive' as any other kind, as intuitive as seeing that one of two sensibly presented lines is the longer. Experimental psychologists have not completely neglected such facts, *e.g.*, Boring wrote 'The immediate perception of duration is very easy to demonstrate experimentally. A device is arranged to give electrically three successive clicks, defining two successive intervals. . . . The duration of the intervals is variable. . . . The observer is asked to say which interval is longer, and he makes a correct judgment with the greatest ease' (*The Physical Dimensions of Consciousness*, p. 133). Boring adds 'To introspection the first interval simply "is there", and the second comes as "too long"'. But notice that, although, in one sense, it is appropriate to say that, for the observer, the first interval "is there", when the second comes as "too long", yet neither the first interval nor its boundaries are being (in my sense) sensibly presented during or after the second interval; so this is a case of attention to what is immediately remembered.

The experiences which, following Russell, we have called 'immediate memory' provide empiricist philosophers with what they have sought when they invoked the specious present doctrine, for in such experiences temporal relations—precedence and overlapping—are 'given' in as simple and direct a manner as that in which spatial relations are given in visibly presented fields. Furthermore, Broad's picture of a characteristic called 'presentedness' dwindling from a maximum to zero seems, in

some ways, applicable to the experiences in question, in that one's access to what has just been sensibly presented is transient and seems to be lost gradually. If I try to go on attending to a group of sounds for as long as possible, I reach a stage when I no longer seem to be retrospectively inspecting the sounds, but to be accepting propositions about them, and perhaps imaginatively reproducing them. But the transition is gradual—I could not have indicated any moment as the precise time when the change became complete. In Broad's second model, the base of the triangle represented the duration of the specious present (= the duration of the sense-contents which have some degree of presentedness). If this model is to be cashed in terms of what we have called 'immediate memory', it can be criticised for representing as determinate what is phenomenologically indeterminate.

To sum up this section—there are at least two methods of cashing the specious present doctrine, either of which gives the empiricist what he wanted from it, *i.e.*, a way of ostensively defining our temporal concepts. The first method is by reference to what is sensibly presented, the second by reference to what is immediately remembered. For reasons already explained, the first method has been illustrated only in terms of visual experience. The second method has been illustrated only in terms of auditory experience. This is because it so happens that I am able to 'inspect' recent sounds more easily, and for longer, than I can 'inspect' recent sensations of other kinds. This may be an idiosyncrasy, but Russell, I notice, also confines himself to auditory experience in illustrating immediate memory.

IV

It might be thought that, if the foregoing account is accepted in substance, it is immaterial whether we continue to use the term 'specious present'. I do not share this view. It seems to me essential to examine the implications of using this term, before deciding whether to retain it.

First let us ask why philosophers, who accept James's view that the contents of one's immediate experience have a duration, should have acquiesced in describing this duration as the '*specious* present', instead of calling it, say, the 'conscious present'. The reason why E. R. Clay, who introduced the term, used the word 'specious' is made clear in the passage quoted by James (*Principles*, p. 609)—'The relation of experience to time

has not been profoundly studied. Its objects are given as being of the present, but the part of time referred to by the datum is a very different thing from the conterminous of the past and future which philosophy denotes by the name Present. The present to which the datum refers is really a part of the past—a recent past—delusively given as being a time that intervenes between the past and the future. Let it be named the specious present. . . . Time, then, considered relatively to human apprehension, consists of four parts, *viz.*, the obvious past, the specious present, the real present, and the future.'

Clay was assuming, as many others have done, that the strict, or philosophically correct, use of 'present' is to refer to a boundary ('conterminous') between past and future, a boundary conceived as a 'point-instant'. He considered it a loose use of 'present' to apply it to a duration. Hence the picture of time as a line of which the 'specious present' is a segment and the 'real present' is a boundary of this segment. The philosophers who have adopted the term 'specious present' do not usually make it clear whether they are subscribing to Clay's view regarding the correct use of 'present'. Broad [1] apparently does accept this view, but its implications are, I think, paradoxical. It takes time to utter the word 'present' (or 'now'), and a longer time to utter sentences containing such words. Hence, in order to use 'present' in its allegedly correct sense, for purposes of communication, one would require a convention to the effect that the moment indicated is that of the earlier (or later) boundary of the noise-token 'present', or of the sentence containing it. Plainly we do not, even as philosophers, employ any such convention. The fact that it is convenient for physicists and mathematicians to give a *determinate* value to t (= time-interval) in their equations, should not lead us to the conclusion that only an 'instantaneous event' can strictly be described as present. Professor J. N. Findlay [2] has recently protested forcibly against this view as to the philosophically correct use of 'present'. He argues as follows—'Now in regard to time it is plain that there is a strong tendency in language to use terms connected with the 'present' in an ever stricter manner. . . . Our use of the present tense and of the temporal adverb 'now' is not very strict in ordinary circumstances: we are prepared to say, even of happenings that last a considerable time, that they are happening *now*, *e.g.*, we say

[1] vd. *Examination of McTaggart's Philosophy*, Vol. II, Part I, p. 283.
[2] *Logic and Language* (first series), edited by A. G. N. Flew, p. 41 *et seq.*

'The National Anthem is now being sung', 'The Derby is now being run', etc. Now the present tense and the temporal adverb 'now' *might* have been the sort of speech-form that we tended to use more and more widely, so that we might easily have been persuaded to say 'The history of England is now running its course', 'The heat death of the Universe is now taking place'. We might then have been persuaded to allow that, since a *whole* cannot be happening now, unless all its component *parts* are happening now, John is now really signing Magna Carta, life on earth is now really extinct, and so on. . . . The natural development of the speech forms we are considering does not, however, lie in this direction. We tend rather, if pressed, to use . . . 'now' more and more narrowly: thus if we had said that the National Anthem was being sung, and someone asked us 'But what are they singing *just now*?', we should not widen our reference to cover the whole evening's concert, but narrow it to apply to some line or phrase or word or note of the National Anthem. Now since our tendencies lie in *this* direction . . . it is possible to force us to grant that the really strict speaker would not use these forms of speech in the case of anything but a happening which was so short that it took *no time at all*.'

I have quoted this passage at length because, although I agree that the view Clay adopted as to the philosophically correct use of 'present' is absurd, Findlay's argument presents a challenge to the viewpoint I have been adopting in this paper. For Findlay seems to be assuming that it is entirely arbitrary how 'wide' we make the 'reference' of words like 'now' or 'present'; and that although there is in fact a tendency (which he ascribes to 'language') to make progressively narrower the reference of such words, this is an accident, for the tendency might have worked in the opposite direction. He seems to assume that if I say 'The Derby is now being run', 'now' is used to refer to the duration of that race; and that the reference of 'now' would be wider—would indeed be the total duration of the Universe—if we decided to use it in 'The heat death of the Universe is now taking place'. But this last sentence is surely perfectly good English, though obviously a person who used it would not be using 'now' to refer to the duration of the Universe. When we speak of 'the present century' we are not using 'present' to refer to a duration of 100 years. Surely 'the present century' means 'the century which overlaps with the present'; and surely 'The heat death of the Universe is now taking place' means something like

'The entropy of the Universe is increasing at (or during) the present, and will continue to increase until life is extinct'; and by 'the present' (in the analysanda), I mean the duration of the so-called specious present, as cashed in one or other of the ways described earlier. It is gratuitous, and, I think, fallacious, to infer from our idioms that the reference of 'now' or 'present' may be indefinitely wide (*i.e.*, that the period referred to by 'now' may be indefinitely long). All of the apparently wider uses can, and, I suggest, should be analysed in such a way that 'now' or 'present' refers to the duration of what is being sensibly presented and/or immediately remembered. If this is granted, it explains the fact that we tend, when pressed, to do what Findlay describes as using 'now' more narrowly. The fact that 'our tendencies lie in *this* direction' is not, then, an accidental feature of our language.

To return to the question raised at the beginning of this section—whether we should retain the term 'specious present'—I have rejected Clay's reason for adopting this term. This, however, does not dispose of the question. *If* we cash the so-called specious present doctrine by reference to our awareness of very recently terminated sounds or sights, etc., and *if* we adopt the term 'immediate memory' to describe such awareness (as I did in the last section), then it *is* appropriate to describe the duration of what is inspectable in this way as a '*specious* present'; for to speak of 'remembering' so-and-so implies that so-and-so is past. In that case, we ought to use 'present' to refer to the date of what is being sensibly presented. Now the philosophers who have invoked the specious present doctrine have usually, if we judge by their illustrations, been cashing the doctrine in terms of our awareness of recently terminated sounds or sights, *but* they have usually spoken of the contents or objects of such awareness as being 'sense-given', or (like Broad) as 'sensa'. Now unless its implications are made explicit and accepted, this procedure is bound to cause confusion. It invites the sort of criticism which Paton directed against Broad in his paper on 'Self-Identity'—'surely the sensa must be at the time they are sensed, and it is impossible to sense sensa which are earlier than the time at which they are sensed' (*In Defence of Reason*, p. 106). For your audience is liable to assume, as, I take it, Paton did, that terms like 'sense-given', 'sensed', etc., are being used as equivalent to 'sensibly presented' as I have been using the latter, distinguishing what is 'sensibly presented' from what is 'immediately remembered'. And if one assumes

this, one will be unable to reconcile the examples one is offered with the doctrine they are supposed to illustrate; and one will naturally protest that the recent positions of the second-hand are not sense-given, or that while one pip of a time-signal is being sense-given the preceding pip is not.

The sensum-terminology could, of course, be adapted to avoid this confusion, by ruling that 'x is being sense-given (sensed)' is to be used not only when x is (in my usage) being sensibly presented, but also when x is (in my usage) being immediately remembered. If a critic then protests that a sound which has audibly terminated is past, and that it would be absurd to speak of 'sensing' what is past, this would presumably be because he is using 'present' to refer to the date of what is (in my sense) being sensibly presented. But 'present' could equally well be used to refer to the (vaguely bounded) duration of what is, in our proposed extended sense, being 'sense-given', and 'past' should then be used only in the correlative sense. If this policy were adopted, the term 'specious present' should be dropped. It would be inconsistent (or, at least, misleading) to define 'sense-given', 'present' and 'past' in the manner I have just indicated, and to continue to describe the duration of what is then said to be sense-given as a '*specious* present'. To do this would be to tacitly admit (or, at least, to suggest) that 'present' ought (strictly) to be used in a narrower sense than the one that has been deliberately allocated to it; and to dither about one's use of 'present' is bound to produce confusion—at any rate when one is discussing the phenomenology of time-perception.

To sum up; the facts in terms of which the so-called specious present doctrine has usually been cashed are the experiences which Russell calls 'immediate memory'. Two alternative terminologies are available for describing the doctrine, as cashed in this way. If we drop Russell's term 'immediate memory' and describe the contents of such experiences as being 'sense-given', we ought to drop the term 'specious present'. If we retain Russell's term 'immediate memory', it would be appropriate to retain the term 'specious present', though we could instead use 'span of immediate memory'. But I recommend that, in any case, we drop the term 'specious present' in favour of 'conscious present', since the latter, unlike the former, would be equally appropriate for either terminology.

I have not considered in detail the vague and shifting distinctions between 'present' and 'past', or between 'hearing', 'seeing', etc., and 'remembering', which are reflected in

everyday conversation. I doubt whether any important contribution to the topics I have discussed would result from so doing. For practical purposes we very rarely have occasion to draw distinctions such as that between being sensibly presented and immediately remembered. It does not, I think, follow that such distinctions are improper or unnecessary for certain theoretical purposes.

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IV.—ON THE PROJECT OF A UNIVERSAL CHARACTER

By Jonathan Cohen

I

During the last thirty years or so the practice has grown up among logicians of attributing the project of a universal character to Leibniz alone among seventeenth century thinkers. This attribution is to be found, for instance, in L. S. Stebbing's *Modern Introduction to Logic*,[1] in Cohen and Nagel's *Introduction to Logic and Scientific Method*,[2] in M. Black's *Nature of Mathematics*,[3] in J. H. Woodger's *Axiomatic Method in Biology*,[4] and in O. Neurath's introductory article in the *International Encyclopaedia of Unified Science*.[5] And it dates, I suspect, from the publication of C. I. Lewis's *Survey of Symbolic Logic* in 1918. Lewis mentioned that Leibniz acknowledged a debt in this connexion to Raymond Lully, Athanasius Kircher, George Dalgarno and John Wilkins. But he considered their writings contained "little which is directly to the point".[6] In this Lewis was obviously right with regard to Leibniz's conception of a calculus of reasoning, but wrong, as I shall try to show, with regard to the project of a universal character, which seems in fact to have been an intellectual commonplace in seventeenth-century Western Europe. This somewhat neglected by-way of philosophical history is worth a brief review, I think, not only in order to fix more precisely the respect in which Leibniz was the only seventeenth century precursor of modern symbolic logicians, but also because it draws attention to an early widespread philosophical muddle about the construction of artificial languages.

Lewis's summary of Leibniz's own ideas, based on L. Couturat's *La Logique de Leibniz*, is quite sufficient to show how much Leibniz's project had in common with those of previous thinkers on the subject. His universal character, in which he was enthusiastically interested from the age of eighteen (1664), was to fill three main roles. It would be what modern linguists call an "international auxiliary language", enabling men of different nations to communicate with one another. It would

[1] Pp. 483 ff. [2] P. 112. [3] P. 17.
[4] P. 13. [5] Vol. i, no. 1, p. 16. [6] P. 5, n. 5.

provide what Lewis calls a "logistic" treatment of science in general, a simplified system of symbolism for the exact expression of all actual and possible knowledge. And it would serve as an instrument of discovery and demonstration. The first two purposes could be achieved on Leibniz's view by devising a notation in which each single or basic symbol represented a simple concept and complex notions were expressed by combining the appropriate symbols in one way or another. The number of fundamental symbols would thus be as small as the number of primitive concepts in human thought, so that the notation, together with its vocalisation, could be learnt in a few weeks. Moreover, throughout the world science would be developed in the form of a single, unified encyclopaedia, scientists would be able to attain the same degree of rigour in metaphysics and morals as in geometry and analytics, and nothing chimerical would ever be written down. However, Leibniz never did much himself to develop this system of symbolism though he was constantly urging others to do so. He elaborated only a few of the many analyses of complex notions which would be required in order to establish which concepts should be regarded as primitive. And he learnt enough about two systems of notation to see that they would not serve his purpose. The earlier of these was based on the division of concepts into classes in accordance with their degree of complexity. Any concept could then be symbolised by a fraction of which the denominator indicated the number of the class and the numerator the number of the concept in that class. Later Leibniz considered symbolising primitive concepts by prime numbers and complex concepts by the appropriate product, so that logical synthesis would be represented by arithmetical multiplication and logical analysis by resolution into prime factors.

But besides being an international and scientific language Leibniz's universal character was also to serve as an instrument of discovery and demonstration by exhibiting the implications of what was already known. It was to see into the "inner nature" of things like a "new telescope", and guide our reasonings like an "Ariadne's thread". Leibniz attempted to devise a calculus which would operate on the formulae of his character to this end. It is these fragments of a calculus, expressed in a notation of concept variables and logical constants, and not at all restricted in possible application to Leibniz's own character, which justify the claim that Leibniz anticipated Boole. I shall try to show that, apart from some minor details, it is only this conception of a logical calculus as an ancillary to his universal character

which distinguishes Leibniz's thought on the subject from that of some of his contemporaries and predecessors. Several of these projected, and some completed, a universal character which they hoped would fill the same three roles as Leibniz had in mind, constituting at once a medium of international communication, a simplified notation for science, and a method of discovery and demonstration.

II

The need for an international auxiliary language was widely felt in seventeenth century Europe for several reasons. The learned were increasingly using their own several vernaculars instead of Latin, merchants faced an immense linguistic problem on the new trade-routes, and missionaries were confronted with the same difficulties. Consequently the recent discovery that written Chinese was used in the Far East as a means of intercommunication by peoples whose spoken languages differed greatly from one another was of great interest. Indeed, it set the pattern for almost all seventeenth-century speculation on these matters, and was referred to by almost all who discussed them. The ideal was not a new spoken language which could also be written (like Lingua Franca or Pidgin-English) by a system of phonograms, but a written language constituted by a new system of ideograms which could also be spoken. As such it was intended to be something quite different from the ciphering of words or letters practised by cryptographers even in the ancient world. The programme for its construction was first enunciated by Francis Bacon in *The Advancement of Learning*[1] in 1605, where he speaks of the project as "the mint of knowledge" since "words are the tokens current and accepted for conceits, as moneys are for values". What he seeks is a system of "real characters" which, as in China, would "express neither letters nor words in gross, but things or notions" and would be "as many, I suppose, as radical words".

Bacon mentioned this project again in *De Augmentis Scientiarum*[2] in 1623, where he added that "any book written in characters of this kind can be read off by each nation in their

[1] *Philosophical Works*, ed. J. M. Robertson, 1905, p. 121. Although written English was a language of phonograms in the seventeenth century (N.B. how in his *Essay, e.g.*, Locke treats words as sounds but not as marks, III, ix, 2, IV, viii, 13, xi, 7, xxi, 4, etc.) and had not yet become the ideographic language into which the standardisation of its spelling later transformed it, I have sometimes modernised the speiling of my quotations so far as this facilitates understanding them to-day.

[2] *Ibid.* p. 522.

own language". But he was not enthusiastic over its utility, apparently because of the inconveniently large number of characters he thought it would require. Descartes thought he could circumvent this difficulty when Mersenne informed him in 1629 of a similar project, formulated apparently by a Mr. Hardy.[1] As soon as "the true philosophy" was known, the "clear and simple ideas" which are the basis of good science could be discovered, enumerated and arranged. It would then be possible to devise a system for symbolising these so that the most complex notions could be as easily expressed as the largest numbers, because an order would have been established among all possible human thoughts similar to that prevailing naturally among numbers, and the whole language could be learnt in a few days. Above all, this language would represent everything so clearly to the human judgment that error would be almost impossible and even peasants would be better able to assess the truth than can philosophers at present.

Leibniz actually took a copy of this passage,[2] and added to it the comment that "although this language depends on the true philosophy it does not depend on its perfection". It could be established while philosophy was yet imperfect and grow as knowledge grew. "Meanwhile it would be a wonderful help for preserving what we know, seeing what we lack, discovering the means of attaining this, and above all for settling controversies where the correctness of a chain of reasoning is at stake". But there were others before Leibniz who thought that the construction of a universal character on mathematicist principles need not wait for the scientific millennium.

Indeed the project of a universal character of some sort became a commonplace from Descartes' time onwards, and the languages seem to have been designed as often on Baconian as on Cartesian lines. To the former type probably belonged the scheme drawn up by William Bedell, Bishop of Kilmore, in 1633, which he persuaded a Reverend Johnston to execute. The results of their work were unfortunately lost in the Irish rebellion of 1641.[3] Of this type too were the projects of the elder Vos and Herman Hugo,[4] and also those of Philip Labbé[5]—

[1] Letter to Mersenne of 20 November, 1629, *Oeuvres*, ed. C. Adam and P. Tannery (1897), vol. i, p. 76.

[2] *Opuscules et Fragments Inédits de Leibniz*, ed. L. Couturat (1903), p. 27.

[3] G. Burnet, *Life of William Bedell* (1688), p. 79.

[4] J. Wilkins, *Essay towards a Real Character and a Philosophical Language* (1668), p. 13.

[5] O. Funke, *Zum Weltsprachenproblem in England in 17 Jahrhundert*, 1929, p. 53.

and Edward Somerset, second Marquis of Worcester.[1] At Ipswich in 1657 Cave Beck published a grammar and dictionary of such a language, entitled *The Universal Character, By which all the nations in the world may understand one another's Conceptions, reading out of one Common writing their own Mother Tongues.* In his preface he remarked "This last century of years, much hath been the discourse and expectation of learned men, concerning the finding out of a universal character, which if happily contrived, so as to avoid all equivocal words, anomalous variations and superfluous synonomas . . . would much advantage mankind in their civil commerce, and be a singular means of propagating all sorts of learning and true religion in the world: such a character being to be learned in as few weeks as the Latin tongue usually requireth years to be perfect in." Similar works were published by J. J. Becher at Frankfurt in 1661 (*Character, pro notitia linguarum universali*), and by Athanasius Kircher at Rome in 1663 (*Polygraphia Nova et Universalis*), who provided dictionaries into and out of his language for Latin, Italian, French, Spanish and German. Beck, Becher and Kircher all make much use of numerals in their notation, which is mostly built up by numbering English or Latin words in their alphabetical order; and Becher and Kircher acknowledge a debt to the cryptographic researches of Johann Tritheim, published early in the sixteenth century under the title of *Polygraphia*. The syntax, accidence and word-order of their languages is, roughly, the highest common factor of Romance, Teutonic and Semitic grammars, and their dictionaries presuppose a one-one correspondence of meaning between the vocabularies of all ordinary languages. So that in effect their languages are systems for ciphering a limited group of languages on a unitary pattern. No Japanese, for instance, could read his vernacular directly out of their sentences, though he could no doubt learn to translate them. But each of these authors nevertheless claimed to be expounding a universal real character, in Bacon's sense (like the modern international code of nautical signals) and not a mere cipher of limited application.

Mersenne seems to have been the first to design a universal language of the Cartesian type, for he mentions this achievement in a letter of 1636 or 1637.[2] He was soon followed by

[1] *A century of the Names and Scantlings of such inventions as at present I can call to mind to have tried and perfected*, 1663.

[2] Adam-Tannery, *op. cit.*, vol. i, p. 572.

F. Lodwick,[1] a London merchant, J. A. Comenius,[2] the Czech educationalist, and Sir Thomas Urquhart, the translator of Rabelais. Mersenne did not describe his system in detail, and there seems no evidence that Comenius ever worked out any details. But Lodwick wrote two books on the subject. The first of these was published in 1647 as *A Common Writing, whereby two, although not understanding one the other's language yet by the help thereof may communicate their minds one to another.* and the second, which was largely a refinement of the first, appeared in 1652, as *The Groundwork, or foundation laid (or so intended) for the framing of a new perfect language and an universal or common writing.* Lodwick's characters are made up of signs for a large number of basic roots and of "distinctional marks" around each sign which signify the derivative form required. Some of these derivative forms, called "abbreviatives", consist of a small range of variations around each radical notion. Its contradictory, for instance, can be signified in this way. The other derivative forms arise from the inflexions necessary to produce a verb, noun, adjective and adverb, with a large range of moods, tenses, cases, etc., from each abbreviative. The resulting scheme is tidier and more systematic than, say, Beck's. And in 1652 Lodwick was clearly of the opinion that "the proper names of things to give them signification is the work, we suppose, of a sound philosopher, who from the knowledge of things and their order in nature, should give them names accordingly, describing that in them by their name, by which in the naming they may be known". Lodwick was convinced too that his character would "much assist the true knowledge of things which is at present much hindered" by verbal ambiguity and vagueness, and he pointed out how it would be of greater use to science than to poetry or rhetoric.[3] But his published scheme was still very far from the degree of simplification which Descartes thought possible. Moreover, it was only an outline, for Lodwick did not, like Beck, provide a lexicon for his Language.

Although Lodwick was interested enough in the problem of

[1] *Cf.* D. Abercrombie, "Forgotten Phoneticians", pp. 3 ff., in *Transactions of the Philological Society* (1948). I am indebted to Mr. Abercrombie for drawing my attention to Lodwick. Some further references to seventeenth century English discussions of the project are to be found in an article by R. F. Jones in *Journal of English and Germanic Philology*, xxxi (1932), p. 315 ff.

[2] Mentioned in a broadsheet of G. Dalgarno's, in MS. Sloane 4377 in the British Museum. *Cf.* J. A. Comenius, *A Pattern of Universal Knowledge* (1651), tr. J. Collier.

[3] In his notebook, MS. Sloane 897.

phonograms to devise a new phonetic notation, which was published in the *Philosophical Transactions* of the Royal Society in 1686 under the title of "An Essay towards an Universal Alphabet", his universal language was ideographic. And Urquhart seems to have been the only seventeenth-century thinker who based his new language on phonograms. Unfortunately most of Urquhart's manuscript was lost at the battle of Worcester in 1651. But an "Introduction to the Universal Language" was saved, and published the next year.[1] Urquhart claimed that he had "couched an alphabet materiative of all the words the mouth of man, with its whole implements, is able to pronounce", and brought "all these words within the system of a language, which, by reason of its logopandocie, may deservedly be intituled *The Universal Tongue*". So that "there is not a word utterable by the mouth of man, which, in this language, hath not a peculiar signification by itself". Indeed, "as, according to Aristotle, there can be no more worlds but one, because all the matter whereof worlds can be composed is in this; so there can be no universal language but this I am about to divulge unto the world, because all the words enunciable are in it contained". Yet "this world of words hath but two hundred and fifty prime radices upon which all the rest are branched". Urquhart had noticed that "no language ever hitherto framed hath observed any order relating to the thing signified . . .; for if the words be ranked in their alphabetical series, the things represented by them will fall to be in severall predicaments; and if the things themselves be categorically classed, the words whereby they are made known will not be tyed to any alphabetical rule". This "imperfection", however, would be remedied in his own language, where there would be "a proportion between the sign and the thing signified". Accordingly, among the sixty-six advantages which Urquhart claimed for his scheme, he asserted that "sooner shall one reach the understanding of things to be signified by the words of this language, than by those of any other, for that as logarithms in comparison of absolute numbers, so do the words thereof in their initials respectively vary according to the nature of the things which they signify". Moreover, "for definitions, divisions and distinctions, no language is so apt; . . . for the affirmation, negation and infinitation of propositions, it hath properties unknown to any other language, most necessary for knowledge; . . . and . . . in matters of enthymems, syllogisms, and all

[1] In *Eskybalauron*, 1652. Republished with slight alterations in *Logopandecteision*, 1653.

manner of illative ratiocination, it is the most compendious in the world".

In 1653 an anonymous Spaniard is said to have published at Rome a scheme for a universal language on Cartesian lines, which he proposed to achieve by dividing all concepts into classes and sub-classes and numbering both the concept-class and the concept within its class.[1] And by 1654 a universal character on Cartesian lines had also been projected by Seth Ward, professor of astronomy at Oxford and previously a lecturer in mathematics at Cambridge, who thought it "a misfortune to the world, that my Lord Bacon was not skilled in mathematics, which made him jealous of their assistance in natural enquiries". Ward, like Descartes, thought that the number of different characters requisite for a universal language on Baconian lines would be so large "that the tradition of learning or facilation of it would be but little advanced by this means. But it did presently occur to me, that by the help of logic and mathematics this might soon receive a mighty advantage, for all discourses being resolved in sentences, those into words, words signifying either simple notions or being resolvable into simple notions, it is manifest that if all the sorts of simple notions be found out, and have symboles assigned to them, those will be extremely few in respect of the other, . . . the reason of their composition easily known, and the most compounded ones at once will be comprehended, and yet will represent to the very eye all the elements of their composition, and so deliver the natures of things: and exact discourses may be made demonstratively without any other pains than is used in the operations of specious analytics."[2]

Ward never developed these ideas in detail, but two friends of his did—George Dalgarno and John Wilkins—though the extent of their debt to him and to each other is in doubt.[3] Dalgarno, an Aberdonian who taught for thirty years at a private grammar-school in Oxford, had been maturing his thoughts on the subject during the later fifties. In a letter to Samuel Hartlib (the publisher-philanthropist and friend of Comenius), for instance, dated 20 April 1657, he criticises—quite rightly—Cave Beck's *Universal Character* as "nothing else but an enigmatical way of writing the English Language", and speaks more favourably of a "treatise" published in 1647, which is

[1] L. Couturat, *La Logique de Leibniz* (1901), p. 51. I have been unable to locate a copy of this scheme.

[2] *Vindiciae Academiarum* (1654), p. 21.

[3] O. Funke, *op. cit.*, pp. 37 ff. Cf. Dugald Stewart's remarks quoted in the Maitland Club's edition of Dalgarno's works (1834), pp. v ff.

XUM

probably Lodwick's *Common Writing*.[1] He published his own *Ars Signorum, vulgo Character Universalis et Lingua Philosophica*, in 1661. In this work he drew up a "Lexicon Grammatico-Philosophicum, seu tabulae rerum, et notionum omnium simpliciorum, et generaliorum, tam artefactarum quam naturalium, rationes et respectus communiores, methodo praedicamentali ordinatas, complectentes". There were seventeen irreducible categories according to Dalgarno, and he designated them by seventeen letters of which each was the initial letter of all names falling under one catogory. The second and third letters of each name signified the sub-class and sub-sub-class to which it referred, and there were provisions for further specialisation where necessary, for instance, in order to deal with the classification of plants. Certain letters could also be prefixed to a word in order to denote its opposite, the mean between two extremes, etc. The language drew no fundamental distinction between different parts of speech, for its author held that all these, even prepositions and interjections, could be derived from his primitive notions (though he made an exception here in favour of six pronouns). Instead, there were rules of word-order designed to show when and how a word was being used adjectivally or adverbially; and inflexions of tense, mood and voice were signified by special suffixes. Dalgarno claimed to have discovered by a process of "analysis logica", applied to ordinary English and Latin usage, that his system adequately represented the normal articulation of human thought. In this respect it is a crude anticipation of the analysis of conversational language practised by symbolic logicians like Reichenbach. But Dalgarno's system is also a precursor of what Lewis calls "logistic". Though he criticised some features of Descartes' philosophy he was thoroughly imbued with its mathematicist spirit. He attacked the Baconian theory, popular among many of his experimentalist contemporaries, that there was no use for a "doctrina praedicamentalis", an arrangement of all knowledge in an ordered system. He held that, while metaphysics and logic constituted a single art, the difference between this art and linguistics was precisely analogous to the difference between the symbolised and its symbol, so that when the two were correlated there would be but one science of them all. Accordingly, Dalgarno believed not only that his universal character would make it much easier for the young to learn the principles of true science and the correct practice of logic, but also that the analyses which it made possible would lead "in penitiorem et interiorem rerum cognitionem". And in

[1] *Cf.* MS. Sloane 4377.

view of the widespread contemporary interest in stenography and in the physiology of speech, sight and hearing, it is hardly surprising that Dalgarno made two further claims for his Character.[1] He asserted that it produced the best short-hand so far invented, and also that it could be used for conveying knowledge to the deaf and dumb. (His specialised work on the latter subject, *Didascalocophus, or the Deaf and Dumb Man's Tutor*, was not published till 1680.)

John Wilkins, Cromwell's brother-in-law and first co-secretary (with Henry Oldenburgh) of the Royal Society, had published as long ago as 1641 a book on cryptography in which he devoted one chapter to the problem of a universal character.[2] In 1668 he published his *Essay towards a Real Character and a Philosophical Language*. This work had been commissioned some time previously by the Royal Society, and would have been published much sooner if London's great fire of 1666 had not destroyed most of the printed sheets and a great part of the manuscript. As befitted its sponsors it was a much larger and more thorough undertaking than Dalgarno's, though on the same general lines. Wilkins was aided in it by three collaborators, expert in botany, zoology and lexicography, respectively. There were now forty irreducible categories or *summa genera* and a vast range of subordinate classifications. A much wider range of ordinary linguistic usage was examined, and as a result Wilkins adopted a distinction, which Dalgarno, though he had himself once drawn it, had come to the conclusion was not fundamental. Wilkins firmly distinguishes in his language between what he calls "integrals" (corresponding to nouns, verbs, adjectives and adverbs) and what he calls "particles" (corresponding to pronouns, conjunctions, prepositions, inflexions, interjections and the copula). There was a much more comprehensive "alphabetical dictionary, wherein all English words according to their various significances are either referred to their places in the philosophical tables or explained by such words as are in those tables". Wilkins' character, like Dalgarno's, could be vocalised in the form of a distinct spoken language, as well as translated into any written or spoken vernacular. But Wilkins also devised an entirely new notation (looking on a printed page something like a cross between Pitman shorthand and ancient Syriac) for writing his character.

Wilkins believed in the same two fundamental requisites for the construction of a universal character as did Dalgarno.

[1] *Cf.* Ms. Sloane 4377.

[2] *Mercury, or the Secret, Swift Messenger*, pp. 105 ff.

The first necessity was " so to contrive the enumeration of things and notions, as that they may be full and adequate, without any redundancy or deficiency as to the number of them, and regular as to their place and order ". The second basic requisite was so to contrive a mark for every thing and notion that these marks should have " a dependance upon, and relation to, one another, as might be suitable to the nature of the things and notions which they represented ", and that thus, " besides . . . helping the memory . . . , the understanding likewise would be highly improved, and we should, by learning the character and the names of things, be instructed likewise in their natures ". Indeed, he felt convinced that his system would " contribute much to the clearing of some of our modern differences in religion, by unmasking many wild errors, that shelter themselves under the disguise of affected phrases, which being philosophically unfolded, and rendered according to the genuine and natural importance of words, will appear to be inconsistencies and contradictions. And several of these pretended, mysterious, profound notions, expressed in great swelling words, whereby some men set up for reputation, being this way examined, will appear to be either nonsense or very flat and jejune ". Thus, while Hobbes (as a result of his knowledge about Chinese) had achieved an *ad hoc* linguistic critique of ontology, for instance, by asking whether those who spoke a language which lacked a verb " to be " would ever use or be worried by the notions of " entity ", " essence ", and the like,[1] Wilkins was apparently proposing to achieve similar deflationary purposes by systematic analysis in terms of a philosophical language. Here again he was following Dalgarno, who had claimed for his own character, in a dedicatory letter, that " veritates philosophicae variae aperientur, v.g. Nullas dare notiones transcendentales, vel extravagantes, ut de uno, vero, bono, fabulantur, potius quam philosophantur metaphysici ". And, like Dalgarno, Wilkins shared neither Descartes' belief that the construction of such a universal character must await the scientific millennium nor Urquhart's self-confidence that he was the linguistic messiah. He hoped that other members of the Royal Society would develop and improve his system. In order to examine its utility the Society appointed a special committee, among the members of which were Robert Boyle, Christopher Wren, John Wallis and Robert Hooke.[2]

[1] *Leviathan*, pt. iv, ch. 46. *Cf.* Leibniz, *Philosophischen Schriften*, ed. Gerhardt (1865-90), vol. iv, p. 145.

[2] Entry in *Journal Book* of the Royal Society under May 14, 1668. There is no trace of any report by this committee in the Society's *Register Book* or in its library catalogue.

III

In view of Leibniz's great interest in the details of Dalgarno's and Wilkins' universal characters it is surprising that he seems to have read their books rather carelessly in one respect. To an example of Dalgarno's character which belonged to him Leibniz appended the comment [1] that, as he "had said to Robert Boyle and Henry Oldenburgh", Dalgarno and Wilkins had not sufficiently grasped the magnitude of the thing and its true use, "for their language or character achieves this alone, convenient communication between those sundered by language, but the true real character, as I conceive it, would be thought one of the most apt instruments of the human mind, with an invincible power for discovery, memory and judgement". Leibniz's misunderstanding in this respect was echoed in 1901 by L.Couturat in his comprehensive study of Leibniz's logic which immediately became the standard work of reference on the subject.[2] It may be that Lewis's error arose from relying too much on Couturat's thoroughness. I hope, however, to have made it clear that in conceiving of a universal character as an important new intellectual tool, with powers not possessed by ordinary languages, Leibniz was anticipated not only by Wilkins and Dalgarno, but also by Ward, Urquhart and Descartes. It is only in thinking of a logical calculus as an ancillary to his universal character which would be useful for eliciting implications that Leibniz seems to have had no precursor. Yet Leibniz, as is well known, was inspired in this direction, not only by reading Hobbes' elliptical remark "Per ratiocinationem intelligo computationem", but also by his early acquaintance with Raymond Lully's *Ars Magna* which Kircher had revised and developed by 1665.[3] Kircher's system, like Lully's, comprised a table of fundamental notions, specially symbolised, from which the permissible combinations of symbols could be read off as theorems. But apparently Kircher did not associate his own universal character or *polygraphia* with this scientific method. After the early publication of his *de Arte Combinatoria* (1666), in which he sought to develop the ideas of Lully and Kircher, Leibniz shows, in several

[1] *Op. cit.*, vol. vii, p. 7. [2] *Op. cit.*, p. 76; *cf.* p. 60.

[3] *Ars Magna Sciendi*, Amsterdam (1669). Cf. Hobbes, *De Corpore*, I, i, 2. Bacon had noted in his *Novum Organum*, II, xxvii (*Philosophical Works*, ed. Robertson, p. 335) that "The mathematical postulate that if two things are equal to the same thing they are equal to one another is conformable with the rule of the syllogism in logic which unites propositions agreeing in a middle term." Cf. Locke, *Essay*, IV, xii, 15 for a speculation on the subject.

discussions of his project for a universal character,[1] too much respect for the heuristic importance of experiment to expect a complete answer to all scientific enquiries along the lines of Lully's method. But in conformity with his doctrine that the predicate of any true proposition is "contained" in its subject, he was far from condemning Lully's art, as Bacon did, as "a method of imposture . . . , being nothing but a mass and heap of the terms of all arts, to the end that they who are ready with the terms may be thought to understand the arts themselves".[2]

In conclusion it may be of interest to review briefly the main reasons why Bacon was correct in his low estimate of the prospects of a universal character, and why the seventeenth-century mathematicists' project of one which should constitute at once a medium of international communication, a simplified notation for science, and a method of discovery and demonstration, was a philosophical muddle and never of the slightest direct help to any merchant or scientist, even though it may indirectly have done something to inspire the later work of men like Linnaeus and Lavoisier. No doubt Kepler's laws, for instance, were not deducible within any of the proposed systems. No doubt a Cartesian misconception of the logic of scientific argument, and an Aristotelian preoccupation with the problems of classification rather than the study of motion, were often major factors in the muddle. But there were also other factors. It was clearly a mistake to think that the same language could serve adequately both as an unspecialised international auxiliary and also as a scientific terminology. J. R. Firth[3] has claimed, indeed, that Descartes envisaged two kinds of universal language, a philosophical one based on a logical system and an ordinary one for "esprits vulgaires". But Descartes' remarks on the subject do not lend themselves to this interpretation. He clearly thought, like Urquhart and the others, that any language would be far easier to learn if it were isomorphic with the facts of nature as systematised by science. Dalgarno even reproduced in one of his broadsheets a certificate, dated May 1658, from Richard Love, then professor of divinity at Cambridge, to the effect that two young bachelors of arts, after spending two hours a day for a fortnight in the study of his character, had learnt to communicate with one another in that language and to translate anything into, or out of, it.

[1] *Eg.* in a letter to Oldenburgh, *op. cit.*, vol. vii, pp. 11 ff.
[2] *De Augmentis Scientiarum*, bk. vi, ch. ii (*op. cit.*, p. 533).
[3] *The Tongues of Man* (1937), p. 70.

Now, the structural simplification which serves an international auxiliary best, of course, is a sweeping abolition of grammatical inflexions—a simplification which all these languages were far from carrying out. But, when words are constructed on a system parallel to the scientific classification of the things to which they refer, their meanings are more, not less, difficult for non-scientists to avoid confusing. Moreover, though naming, description, classification, generalisation, reasoning, explanation and prediction are the main tasks of scientific speech and writing, no-one would now deny that these are far from being the only purposes which we require an everyday language like an international auxiliary to serve. But the range and flexibility of meaning which we expect from an unspecialised vocabulary is quite inappropriate for a taxonomic notation, say, like that of modern chemistry. Again, scientific theory is best served by a language which carries over the least possible irrelevant associations from non-scientific discourse. But an international auxiliary is most easily learnt if its radicals are already familiar and these are therefore best drawn from words already in international currency, as in L. Hogben's Interglossa.

Finally, while an unspecialised language must be capable of application to all fields of human experience, it has yet to be shown that the ideal of a systematic terminology can usefully operate not only at the level of each individual science but also at the level of science as a whole. We can organise the richness of our non-scientific vocabulary into tables of synonyms, antonyms, etc., like those provided by Roget's *Thesaurus*, which acknowledges Wilkins's priority in its field, and this may be useful for writers or cross-word solvers, say, who are at a loss for a word. Or we can minimise the number of words and grammatical rules in an unspecialised language, as in Basic English, and this may make it easier for foreigners to learn. Or we can design language-patterns in the course of what R. Carnap [1] calls "pure syntax" and "pure semantics", and from the study of these formal structures, as from other methods of analysis, we may be able to discern logical principles—"general syntax"—underlying all possible languages. Or we can construct an interpreted axiom-system for some part of natural science, as J. H. Woodger [2] has tried to do for embryology, genetics and biological taxonomy. This, as Woodger claims, may for a time give order to the experimentalist's records and direction to his activities. But

[1] *Logical Syntax of Language*, tr. A. Smeaton (1937), p. 7, and *Introduction to Semantics* (1942), p. 1.

[2] *The Axiomatic Method in Biology* (1937).

if we try to do all these four things at once we may fail as badly as Dalgarno and the others failed, since projects like Roget's *Thesaurus* and Basic English, on the one hand, and Carnap's or Woodger's constructions, on the other, have little in common. The former are bound to accept unquestioned the tasks which a language is called upon to perform and are concerned instead to regulate in one way or another the shapes and sounds with which it performs them, while the latter are interested primarily in systematising the tasks which some or all languages can or should perform and are comparatively indifferent to the shapes and sounds which are used for this. Moreover, unless the unity of science, in some important sense, can be shown to be more than a metaphysical speculation or a restrictive programme, it is not surprising to find that in practice scientists prefer to follow Bacon's principle that " uniformity of method is not compatible with multiformity of matter ".

Accordingly, the impossibility of combining a practicable international auxiliary with an adequate scientific terminology has been stressed in this century both by linguists and by scientists.[1] When, for instance, Peano published the fifth volume of *Formulaire de Mathématiques* (1908) in Interlingua it was only his meta-language that was this uninflected Latin. But, since some philosophers [2] are still prone to talk about the properties of " a perfect language " as if the criteria of linguistic perfection were a uniquely determined set of compatible ideals, it is perhaps worth while pointing to the errors of the seventeenth century as an illustration of this phrase's misleading character.

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[1] *E.g.* W. Ostwald in *International Language and Science* (1910), p. 55, and J. H. Woodger in *International Encyclopaedia of Unified Science*, vol. ii, no. 5, pp. 1 ff.

[2] *E.g.* B. Russell, *Introduction to Mathematical Philosophy* (1920), p. 183, but *cf.* " Logical Atomism ", in *Contemporary British Philosophy*, First Series (1924), p. 377; *cf.* also J. R. Weinberg, *Logical Positivism* (1936), p. 25.

V.—DISCUSSIONS

THE PARADOXES OF DERIVED OBLIGATION

In his article in Mind for January, 1951 on 'Deontic Logic', and in chapter v of his *Essay in Modal Logic*, Dr. von Wright draws a number of parallels between statements of obligation and permission and ordinary modal assertions. Two which he omits, and which seem to me instructive, are the deontic analogues of the paradoxes of strict implication.

The deontic analogue of strict implication itself is what von Wright calls being 'committed' by the doing of A to the doing of B, and symbolises as 'O(A → B)'. He defines this as '∼ P(A &∼B)', *i.e.* 'It is not permitted to do A without doing B'. Now it is a law of his system that if any act is forbidden, then it is likewise forbidden to do that act in conjunction with any other act or omission to act whatever. Symbolically, ∼ PA → ∼ P(A & B) and also ∼ PA → ∼ P(A& ∼ B). But the definition of 'O(A → B)' immediately turns the second of these into '∼ PA → O(A → B)'. That is, the doing of what is forbidden commits us to the doing of anything whatsoever. (Stealing, *e.g.*—supposing that to be forbidden—commits us to committing adultery; and also, of course, to not committing adultery.) Again, if the omission of any act is not permitted, *i.e.* if the act is obligatory, then we are 'committed' to it by any act whatsoever. For, by the same principle as before, ∼ P ∼ B → ∼ P(A& ∼ B), *i.e.* ∼ P ∼ B → O(A → B), *i.e.* OB → O(A → B).

With the second of these we need not further concern ourselves; it is the first which is more obviously worrying. It tends rather to suggest a 'might-as-well-be-hanged-for-a-sheep-as-a-lamb' morality. This impression is, of course, illusory; as with the Lewis paradoxes, the paradoxical character of the thesis disappears when the sense assigned by definition to its terms is duly observed. To say that stealing commits us to committing adultery, using 'commits us to' in von Wright's sense, means that stealing accompanied by refraining from adultery is still stealing, and so is still wrong; and it means no more than that. It does not mean that when we have stolen we thereafter stand under an obligation to commit adultery. But a question which we cannot but raise is 'Ought it not to mean that?'

We would normally say that the performance of a wrong act A 'commits us to' the performance of another act B, only if our subsequent failure to do B would be (even if B were in itself morally indifferent, perhaps even if it were bad) an *additional* wrong, over and above the original wrong of doing A. Whether this feature of our ordinary notion of commitment can be introduced

into a formalised logic of deontic modalities, I do not know. But it is interesting that von Wright himself, when comparing strict implication with entailment (*Essay*, p. 9), says that 'entailment, contrary to what might have been expected, is not a purely modal idea'. This may well be true of the analogous conception of commitment.

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A NOTE ON MEANING AND VERIFICATION

H. FEIGL, in an historical review[1] of the doctrines of Logical Positivism, said that "the most frequently misunderstood regulative principle used by them is the criterion of factual meaningfulness". If I have not enormously misinterpreted the recent article "On meaning and verification" (MIND, Jan., 1953) by Mr. J. L. Evans, here is another example of this misunderstanding in that he both attributes to the Logical Positivists views which they never held, and considers as novel views which have been their stock-in-trade for many years. I will illustrate this by quotations from a very few well known Positivist writings.[2]

(*a*) Mr. Evans says that the verification principle is "a disguised attempt to give a simple all-embracing answer to the question 'How do sentences mean?'" (p. 13, *cf.* 4, 15). This is not so. Ayer (F.E.K., 93, *cf.* L.T.L. 90) explicitly points out that the question is wrongly posed. Furthermore, Ayer (L.T.L. 15) and Feigl (376) allow that there may be all kinds of meanings and ways of finding them. They are explicitly only concerned with "literal" (Ayer, L.T.L., *passim*, esp. 5, 8, 15, 35, 36, 39, 135, 136) or "cognitive" (Feigl, 379) meaning. And within this they further distinguish "factual meaning" (Feigl, 379 ff., Ayer, L.T.L. 15, 35, 36, 38). Nor did they ever intend that the principle should "be stretched to include a few more statements, especially ethical ones" (Evans, p. 2, *cf.* 17). Ethical sentences, they believe, have no "factual meaning", "they are neither true nor false" (Feigl, 402, Ayer, L.T.L. 22, 103, 107), and therefore do not come under this theory. The analysis of such sentences—which can be regarded as investigating the rules for the use of ethical terms (Feigl, 402, Ayer, L.T.L. 103)—is independent of the previous work (Ayer, L.T.L. 20). Positivists have indeed usually agreed in taking an emotive view, but so have others.

I shall show below that one Positivist, Schlick, who did try to give a general answer to the problem of meaning advanced exactly the same views as Mr. Evans. But Schlick held his at least as early as 1936.

(*b*) Because he has not seen exactly what the Positivists were trying to do, Mr. Evans suggests (p. 16) that the verification

[1] Feigl 381.

[2] These, with their abbreviations for reference purposes, are :—Schlick, *i.e.* "Meaning and Verification", by M. Schlick in Readings in Philosophical Analysis (first published in *The Philosophical Review*, 1936). Feigl, *i.e.* "Logical Empiricism", by H. Feigl in *Twentieth Century Philosophy*, 1943. L.T.L., *i.e. Language, Truth and Logic*, by A. J. Ayer, first published in 1936, with a new introduction in 1946. F.E.K., *i.e. The Foundations of Empirical Knowledge*, by A. J. Ayer, 1940.

principle is " not a criterion of the meaningfulness of sentences . . . it is a suggested criterion by which we can decide whether a given sentence can be classed as empirical or not " (*cf.* p. 18 " The Verification Principle may be regarded then, as an attempt to exhibit the logic of different types of statement; it is not in any sense a criterion of meaning "). But the Logical Positivists have shown explicitly that there is no antithesis here at all. They agree that the " purpose of the criterion is to delimit the type of expression that has possible reference to fact from the other types that do not have this type of significance " (Feigl, 381), and that " one way of describing the use of the principle of verification would be to say that it provided a means of determining when an indicative sentence expressed a proposition or, in other words, of distinguishing the statements that belonged to the class of propositions from those that did not " (Ayer, L.T.L. 8). Schlick said that meaning was given " by applying to a sentence the rules of the logical grammar of our language " (143-144). To discover whether a sentence is "factually meaningful" *is* to discover whether it is an empirical sentence, and this is done by the principle of verification. This is what Ayer had in mind when he talked of " putative propositions " or of sentences which " purport to express propositions " (*e.g.* L.T.L. 31, 38, 41, 116, etc.); the object of the principle being to find out whether they really were so or not. Mr. Evans' conclusion that the Positivist view is " a definition of meaningfulness only so far as this concerns tautologies and empirical sentences " (p. 17) is no more than these Positivists have claimed. Furthermore this antithesis seems at variance with Mr. Evans' own views. Two main points of his article are that " the rules of the use of a word *are* its meaning " (p. 15), and that the rules of the use of a word are the logic of the word (p. 14) and hence the logic of a type of statement (p. 16); from which it seems to follow that the *same* enquiry gives both the logic of a type of statement and a criterion of its meaning.

(*c*) Although Mr. Evans mentions (p. 12) two reasons why a sentence may be meaningless—incidentally Feigl (384) gives five—he thinks it " of the greatest importance " to deny that its unverifiability can be a " cause of its being meaningless ". He says " we don't first attempt to verify or falsify a sentence and, when we fail, conclude that the sentence is meaningless " (*ibid.*). This seems to be a confusion of " verifiable " and " verified " to which Schlick (154) alluded. " You cannot even start verifying before you know the meaning, *i.e.* before you have established the possibility of verification." But the impossibility of verification can be discovered not only by spotting logical fallacies, *e.g.* " my friend died to-morrow ", but also, for example, with a frequent interpretation of theological statements [1] like " God loves men ", by discovering

[1] For some interesting points about the ways of finding the meaning of theological statements, see C. B. Martin, " A Religious Way of Knowing ", MIND, Oct., 1952.

that no possible situation is *allowed* to falsify it. If it purports to be empirical it is then meaningless; though of course if it makes no such pretensions no one claims that the verification principle is relevant. Another example of the same sort is given by Mr. Rees —in the same issue of MIND—in discussing Professor Oakeshott's dictum that "in politics every enterprise is the pursuit of an intimation" (see esp. pp. 71-74). Mr. Evans' mistake seems to be akin to that which Ayer (F.E.K. 92 ff.) attributes to the Formalists. He forgets that the rules of the use of *some* words *may* be "application" rules, *i.e.* given by stating the "kind of situation to which a symbol may be applied" (Ayer, F.E.K. 93, *cf.* Schlick, 147). Though it is not to say that the situation *is* the meaning as the Fido-Fido principle asserts (see Ryle, *Philosophy*, vol. xxiv, p. 73 last paragraph). This is the way in which children often learn the meaning of many words.

(*d*) A minor mistake—though one which makes much of part ii of Mr. Evans' paper inapplicable to the Positivists—is his belief that the Relational theory of meaning is bound up with the verification principle. Ayer (F.E.K. 95-97) explicitly denies the Relational theory, and there is no trace of it in Feigl or Schlick. Carnap's use of it has nothing to do with Positivism.[1] Mr. Evans may have been misled by the opinion—common to empiricists (see paragraph (*c*) above)—that "in all full definitions of empirical terms there is a terminal ostensive step" (Feigl, 380, *cf.* Schlick, 148).

(*e*) But Mr. Evans' main mistake consists in putting forward as a recent change (*cf.* p. 9 "gradually supplanting it")[2] the view that "the meaning of a word is the rules which govern its use". This view is explicitly stated by Ayer (F.E.K. 93, 97), and by Feigl (379) who is giving an historical review of the doctrines of Logical Positivism. But the most explicit and elaborate statement of it known to me was given by Schlick as early as 1936, where he attributes it (148) to Wittgenstein. Schlick says "the meaning of a word or combination of words is determined by a set of rules which regulate their use" (147, *et passim*), and we avoid nonsense "by constructing the sentence in accordance with the rules by which its terms are defined" (155). I cannot see how Mr. Evans' article differs from this. But perhaps he might base his contribution on the remark "whereas we can properly speak of the rules governing the use of words, we should not speak of the rules governing the use of sentences" (p. 9). Undoubtedly these Positivists have spoken of the use both of words and of sentences. Ayer (F.E.K. 97) mentions the use of symbols, where symbols cover words and sentences; Feigl does not explicitly speak of sentences but men-

[1] As Schlick 151, *cf.* 159, pointed out in reference to an earlier work of Carnap.

[2] Far from being novel, it is now respectable enough to be attacked, *e.g.* by F. Waismann in *Analysis*, Oct., 1952.

tions terms and signs as well as words (379 ff.); and certainly Schlick, though he speaks mainly of words, does say "the rules according to which the sentence is to be used" (148, *cf.* 147, 154). However, I cannot think that this difference of Mr. Evans is serious because (1) he himself speaks of "the rules of operation of sentences in use" (p. 18, *cf.* "correct use of certain classes of sentences", *ibid.*, "use for a set of words", "sentences which are used to describe" 16, etc.); (2) his main argument for the distinction is a metaphor about bricks and houses which leads him to say "whereas we can speak of the rules or principles of bricklaying, we cannot speak of the rules governing the use of a completed building". This seems to be simply false. Admittedly the principles of the one are not those of the other, but the other has its principles too; some are called "house-keeping", others "administration", others "town-planning". I also feel doubtful whether the original metaphor is helpful.

I do not, of course, wish to suggest that the Logical Positivists have said the last word on the problem of meaning nor that the letter of their expositions is completely free from ambiguity and inconsistency, but only that some criticisms—of which Mr. Evans' is not the sole example—miss the point and the spirit of their views.

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VI.—CRITICAL NOTICE

Philosophical Investigations. By LUDWIG WITTGENSTEIN. German text and English Translation by G. E. M. ANSCOMBE. Basil Blackwell, Oxford, 1953. 37s. 6d.

THIS book is a treatment, by a philosopher of genius, of a number of intricate problems, intricately connected. It also presents in itself an intricate problem: that of seeing clearly what the author's views are on the topics he discusses, and how these views are connected. The difficulty of doing this arises partly from the structure and style of the book. Wittgenstein himself describes the former accurately in the preface: "The best I could write could never be more than philosophical remarks"; "Thus this book is really only an album". It would, however, be a very strong prejudice against this disregard of the ordinary conventions of exposition, which could survive a careful reading of the whole book. Wittgenstein did not gloss his thoughts; but he arranged them. And the gains in power and concentration are great. It might even be thought that there were good reasons why no attempt at all should be made to present his views in a more conventional form. But this could be true only on a very specialised view of the nature of philosophical understanding. In what follows, I try to trace and connect the main lines of his thought; conscious that, at best, the result must involve a great impoverishment of his rich and complex thinking. I refer to passages in Part I of the book simply by paragraph number (*e.g.* 500); to passages in Part II by section and/or page number (*e.g.* II, xi, p. 200). Quotations from the text are in double quotation marks. Comment and criticism will be interspersed with exposition.

Meaning and Use. In the first thirty-seven or thirty-eight paragraphs of Part I, which are concerned with meaning, Wittgenstein is anxious to make us see "the multiplicity of kinds of words and sentences" (23). We are prone to assimilate different kinds. In particular, we are prone to work with a certain idea of language as consisting of words each correlated with something for which it stands, an object, the meaning of the word (1). This picture, though philosophically misleading for all words, is better suited to some than to others. When we have it in mind, we are primarily thinking of common nouns like 'chair' and 'bread' and of people's names; even primitive reflection shows that it does not fit, say, logical connectives. So not only is there a general tendency to assimilate different kinds of words to each other; there is also a particular direction which this assimilation tends to take. Perhaps the general tendency is in part explained by the fact that words look or sound much alike: from their uniform appearance

on the printed page one would never guess at their diversity of function (11). But there are more complex reasons both for the general tendency and for its particular direction. The central point is this: *the picture with which we are inclined to work derives essentially from the instruction-setting of someone who has already mastered in part the technique of using the language; i.e.* from the situation in which someone is being taught the place of one word, of which he is ignorant, in a way of using language with which he is familiar (*cf.* 10, 27, 30, 32). In this situation, the instructor may well proceed by saying something like 'the word x means (is the name of, stands for, signifies, etc.) y' where the place of 'y' is taken by, *e.g.* 'this' 'this number' 'a number' 'this colour' 'the colour which . . .'; or simply by a synonym or translation of the word in question. In some, though not all, cases, he may accompany these words by pointing. Or he may just answer the question 'What is y (this, this colour, this thing) called?' by pronouncing the word. These procedures may give us the impression of a relation of a unique kind being established between two items, a word and something else; and the further impression that the essence of meaning is to be grasped by the contemplation of this unique relation. To counter this impression, to remind ourselves that the efficacy of these procedures depends on the existence of a prepared framework of linguistic training, we should bear in mind such points as these two: that an ostensive definition (*many* kinds of words can be taught by indicating situations in which they are in some sense applicable) can always be variously interpreted (28); and that the process of asking the names of things and being told them is itself one language-game[1] among others, and a comparatively sophisticated one.

Two minor comments. (1) Perhaps Wittgenstein does not here sufficiently emphasise the point that the *natural* place for the word 'meaning' and its derivatives in ordinary use is in just such instruction-situations as those referred to. One might get the impression that he was saying: 'In philosophy you want the meaning of the word. Don't look for the mythical, uniquely related term, but look at the use; for *that* is the meaning' (*cf.* 43). But in view of the natural place of 'meaning', it might be better to say: 'In doing philosophy, it can't be that you are ignorant of the meaning: what you want to know is the use.' (2) Wittgenstein does not seek to give a complete explanation of why, among all the kinds of names there are, it is substance-names that tend to be taken as the model for meaning. A suggestion which can perhaps be extracted from the text is that (*a*) pointing figures largely both in ostensive explanation of words, and in that more primitive training

[1] Wittgenstein uses this phrase to refer to any particular way, actual or invented, of using language (*e.g.* to a particular way of using a certain sentence, or a certain word); and also to "the whole consisting of language and the actions into which it is woven" (7).

in the naming-game which a child goes through before it actually uses words for any more practical purpose; and (*b*) pointing is more naturally used to discriminate the individual man or horse than any other kind of item. But there remains a question here.

Instead, then, of gazing at this over-simple picture of language, with its attendant assimilations, we are to look at the elements of language as instruments. We are to study their use. Only so can we solve our conceptual problems. Variants on 'use' in Wittgenstein are 'purpose' 'function' 'role' 'part' 'application'. It is not a complaint to say that this central notion is not immediately and wholly clear. The general aim is clear enough: to get us away from our fascination with the dubious relation of naming, of meaning, and to make us look at the speaking and writing of language as one human activity among others, interacting with others; and so to make us notice the different parts that words and sentences play in this activity. But here I inevitably re-introduce one of the variants: 'the parts they play'. Perhaps seeing what sorts of things count as *differences* of use will help one to get clear about the central notion. And here it will seem that there are differences between differences which Wittgenstein might have made more explicit.

Consider first what he says of different kinds of sentences. He makes the point that a formal (grammatical) likeness may cover a functional difference (21-22). Then (23): "But how many kinds of sentence are there? Say assertion, question, and command?—There are *countless* kinds: countless different kinds of use of what we call 'symbols' 'words' 'sentences'." There follows a list of activities which involve the use of language. When we look at the items in the list, it becomes clear that the shift from 'kinds of sentence' in the question to 'kinds of use' in the answer was an important one. The list includes, for example, as separate uses, the activities of reading a story, play-acting, and translating from one language to another. The sentence 'It was raining' might occur in the course of any one of these activities; as it might in a factual narration. It would be absurd to speak of different *sentences* here, let alone of different kinds of sentences. We *might* speak of different uses of the sentence, though it would be better to speak of different linguistic activities in each of which the sentence occurred. Similarly, I suppose, reading aloud a story containing the sentence would involve a different use from copying the story out; reading aloud a translation of a story from reading aloud (*a*) an original, (*b*) a translated factual narrative in all of which the sentence occurred; and there is also the special use involved in sending an old man to sleep by reading aloud from a translation of a play. Surely distinctions are needed here to save the whole notion from sliding into absurdity. Such points as the following call for attention: sometimes there is a formal (grammatical) distinction to correspond to (not to coincide with) a

'difference in use'; sometimes there is not (in what cases would it be more or less natural to have one, and why?); sometimes the existence of a formal distinction would be self-defeating. There is a class of interrogative sentences (sentences of which the standard use is to ask questions); there is no class of translators' sentences: there *could be* no class of copyists' sentences.

Consider next the point that we cannot in general talk about the functions or uses of *words* in the same sort of way as we can talk about the functions or uses of sentences.[1] (Of course, a word may *sometimes* function as a sentence.) To suppose that we could would be like supposing that we could talk of the function of a numeral in the same way as we could talk of the purpose of a calculation; or discuss a gambit and a piece in the same terms. We might imagine a very simple language A, in which a limited number of sentences could be formed from a limited number of words; and a second language B, containing no distinction of sentences and words, but consisting of unitary expressions such that every sentence of A could be translated into a unitary expression of B. Then it might be the case that every remark we could make about a use of a sentence in A would also be true of a use of an expression in B. We could also discuss the uses of the *words* of A; but there would be nothing to be said on this subject in the case of B. What perhaps causes confusion here is that very often when we (and Wittgenstein) discuss the use of *words* of certain classes, what we are concerned with is the *criteria* for their correct application; and this discussion is the same as the discussion of the conditions in which it is correct to use a *sentence* of a certain kind, namely, the sentence which says that we have here a case of what the word in question describes.

The fact that Wittgenstein is content to leave this central notion of use so vague is a manifestation of his reluctance to make distinctions and classifications which are not of direct assistance to the fly in the flybottle (309). Underlying this reluctance is a general, and debatable, doctrine of the nature of philosophy: to which I shall refer later.

Towards the end of these introductory paragraphs there enters one of the main themes of the book (33-36). Wittgenstein imagines an objection to the view that understanding an ostensive definition requires a mastery of the language. Someone might say that all that is necessary to this understanding is for the learner to know what the teacher is *pointing to* or *meaning* or *attending to*, when he makes his gesture—*e.g.* to the shape or to the colour of the object.

[1] *Cf.* Ryle, 'Ordinary Language', *The Philosophical Review*, April, 1953, pp. 178-180. Perhaps Professor Ryle puts too much weight on the fact (if it is a fact) that we do not speak of the 'use' of sentences. Certainly we use them. But it is at any rate true that there are some things we might mean by 'the use of a word' which we could not mean by 'the use of a sentence'; and the other way about.

Wittgenstein does not deny this ; but points out that though there may be characteristic experiences (*e.g.* feelings) of ' meaning ' or ' pointing to ' the shape, which the learner might share with the teacher, their occurrence is not sufficient to make the situation one of ' meaning the shape '. Not just because they do not always occur. Even if they did it would still depend on the circumstances, the setting, on what happens before and afterwards, whether the case is one of intending or interpreting the definition in such and such a way. Seeing this is helped by seeing that there are in fact many things which may occur at the time, and none which must. It is in such cases, however, that we are apt to feign a *special* experience or mental act or process to answer to such a description.

This single topic, of *meaning* or *understanding something* by, say, a gesture, a word or a sentence, is the most persistently recurrent in the book. It is easy to see why. It is a place where two major preoccupations overlap, where two principal enemies may join hands. These enemies are psychologism in the philosophy of meaning, and the doctrine of special experiences in the philosophy of mind. Against them stand Wittgenstein's ideas. To grasp a meaning is to be able to practise a technique ; while ' meaning, understanding something by a word ' is itself a prime instance of those psychological expressions which seem to refer to something which happens at a point, or over a short period, of time, and may indeed do so ; but, when they do, it is to something which gains its significance and its claim to its special title from what stretches before and after the point or the period.

But the main discussion of psychological concepts is deferred until the end of a further discussion of language and logic.

Language, Analysis and Philosophy (38-137). The general over-simple picture of meaning which Wittgenstein examines at the beginning of his book takes an intenser, tauter, form in a certain special doctrine of the *real* names of a language. This doctrine Wittgenstein now discusses ; and the discussion broadens into a general repudiation of the set of philosophical ideas and ideals, roughly indicated by the title of ' logical atomism '. He begins with a consideration of two related notions : the idea of the genuine names of a language, and the idea of the simple indestructible elements of reality which can only be named, not described or defined, and which are the meanings of the genuine names. These are the primary elements referred to by Socrates in the *Theaetetus*, and are identified by Wittgenstein with Russell's ' individuals ' and his own ' objects ' of the *Tractatus* (46). Both ideas are subjected to destructive criticism and diagnosis in 39-59. First Wittgenstein attacks the notion of the word of which the meaning is the object it applies to : he instances the ordinary proper name and distinguishes between its bearer and its meaning (40) ; in this case, too, the meaning is the use (41-43). (Wittgenstein here gives the wrong reason for objecting to the identification of the, or a, meaning

of a proper name with its bearer, or one of its bearers. If we speak at all of the meaning of proper names, it is only in quite *specialised* ways, as when we say that 'Peter' means a stone, or 'Giovanni' means 'John'. This is not an accident of usage, but reflects a radical difference between proper names and other names. But here, as elsewhere, Wittgenstein neglects the use of 'meaning'.)

The antithesis 'simple-composite' is next examined and shown to have application only in a particular context, and to have different applications in different contexts: the *philosophical* question whether something is composite or simple is only too likely to lack a suitable context and hence a sense (47). Next (48-49) a simple model-language is constructed which might be held to answer to the specifications given in the *Theaetetus*; an arrangement of coloured squares is 'described' by an arrangement of letters, one letter for each square, the letters varying with the colour of the squares. We might perhaps say, in the context of this language-game, that the 'simple elements' were the individual squares. But then the assertion that the simple elements can only be named, not described, is at best, perhaps, a misleading way of saying that in the case of an arrangement consisting of one single square, the description consists of a single letter, the name of the coloured square; or of saying that *giving* a name to an element is different from *using* the name to describe a complex. Wittgenstein next considers the doctrine of the indestructibility, the necessary existence, of elements named by genuine names: the doctrine that the very meaningfulness of the name guarantees the existence of the item named. He is rightly not content to rely on a general repudiation of the notion of meaning-as-object, but produces answers to arguments in the doctrine's favour, and further explanations of its sources. The interest of the answers is weakened by the indescribable badness of the arguments (*cf.* 55, 56, 57). The explanations remain. If I understand them rightly, they run as follows. (1) We are inclined to get muddled over sentences like 'Turquoise exists'. This might be used to mean 'There are things which are turquoise'—an ordinary empirical proposition, which could be false—or 'There is such a colour as turquoise' (*i.e.* '"Turquoise" does have meaning as a colour-word'). Taken the second way, it is again an empirical proposition, this time about a word. The muddle begins when we both take it this way and at the same time inconsistently take it as a proposition *using* the word 'turquoise' with the meaning it has, and thus as saying something about the colour, turquoise. Then we seem to be saying something necessary (for a word couldn't be used with, *i.e.* have, a meaning, and not have one), and also seem to be saying something about the colour, *viz.* that it necessarily exists (58). Wittgenstein does not remark that this account might apply to the abstract-noun form of *any* descriptive word (*e.g.* healthiness), and not merely to those which philosophers are prone to take as the names of ultimate elements.

XUM

Perhaps this account should be regarded simply as a supplement to the other (2) which is summed up in the epigram, "What looks as if it *had* to exist is a part of the language" (55). If it were a necessary part of the activity of using a word 'W' that we, say, consulted a sample of W, then we might say that a sample of W must exist in order for the word to have meaning. From this *recherché* possibility, I take it we are to move to the truism that we could not teach a word by naming a sample unless there were a sample to name (*cf.* 50), and to join this to the reflection that there are at any rate some words which it is tempting to suppose could not be learnt except by means of an ostensive definition. Then we have the makings of a tautology which might be misconstrued as an assertion of metaphysically necessary existence.

The doctrine of elements is obviously connected with the belief in *analysis* as the inevitable method of philosophical clarification: the belief that the philosophical elucidation of an ordinary sentence is achieved when it is replaced by another, which makes explicit the complexity of the proposition expressed and reflects exactly the form of the fact described (91). This belief is an illusion, engendered by confusions about language and logic, and to be dispelled only by a clear view of the actual functioning of language. How exactly does the illusion arise (Here Wittgenstein's answer has a kind of passionate obscurity which it is difficult to penetrate.) In logic, which is at once completely pure, exact, and general, we seem to have the clue to the essential nature of thought and language, and with this, the clue to the general *a priori* order of the world, of things empirical (97). For do not thought and language mirror the world? (The *thought* that it is raining is the thought *that it is raining*: the propositions fit the facts (95-96).) Somehow the exactness and order of which logic gives us the ideal must (we think) be hidden in every, even the vaguest-looking, sentence of ordinary language: the *sense* of each must be definite. Logic shows us in advance what the structure of language and the world is—so this structure must be found hidden, to be revealed by analysis, in what we actually say (98 *et seq.*). Wittgenstein does not suggest that what philosophers have called 'analysis' is, in fact, useless. Sometimes "misunderstanding concerning the use of words, caused, among other things, by certain analogies between the forms of expression in different regions of language . . . can be removed by substituting one form of expression for another" (90). But this fact itself contributes to the illusion that there is "something like a final analysis of our forms of language, and so a *single* completely resolved form of every expression" (91).

To dispel this illusion, we are to give up looking for the essence of language and instead are to look at what is all the time before our eyes: the actual functioning of language. Then we see that linguistic activities are as diverse as all the things which we call 'games', and which are so called not because of any single common

element, but because of " family resemblances "—a " complicated network of similarities, overlapping and criss-crossing " (66). (The sub-class of linguistic formations that we call ' propositions ' is also just such a family. We are apt to think we have a clue to the general nature of the *proposition* in the idea of *whatever is true or false.* But one of these ideas cannot be used to elucidate the other : they move too closely together, they share each other's ambiguities (134-137).) Wittgenstein here makes an ingenious double use of his examination of the word ' game '. He not only uses the concept ' game ' to cast light on the concept ' language ' by means of direct *comparison* : games form a family, and so do the various activities which come under the general description of ' using language '. He also uses it as an *illustration,* to cast light on language in another way : by showing the parts which the notions of rules, and of exactness of meaning, play there. Thus the application of the word ' game ' is not limited by any precise boundary ; though a boundary could be fixed for a special purpose. We could say it was an *inexact* concept, that there was an indeterminacy in the rules for its use. But the important thing to notice is that this does not detract from its usefulness for ordinary purposes (and for extraordinary purposes, special rules can be devised). So that it would not even be *correct* in general, to speak of inexactness here ; for ' inexact ' is a word of dispraise, signifying that what is so-called falls short of a standard required for some particular purpose (88) ; and here no such special purpose has been specified. Moreover : (1) in whatever detail we give the rules for the use of a word (or for the playing of a game), cannot we always imagine a case in which there might be a doubt as to whether the rule applied to this case or not ? and (2) is there not always the possibility of someone's not knowing how to interpret the rules or the explanations we give him of them, or the explanations of those explanations ? The point Wittgenstein is making here is that the demand for absolute precision in the rules (a fixed meaning) or for absolute finality in their interpretation, their explanation, is senseless. What determines whether there is enough precision in the rules, or a sufficient explanation of them, is whether the concept is used successfully, with general agreement (84-87).

(One of the illustrations which Wittgenstein uses here is unfortunate. He wishes to say that I could use a proper name " without a fixed meaning ", without its losing its usefulness ; " asked what I understand by ' N ' ", he says, I might adduce various descriptions some of which I might later be prepared to abandon as false of N (79). Of course, I never should be asked *what* I understand by, but *whom* I mean by, ' N ' ; and in answering, I should not be defining ' N ', but identifying N.).

With what conception of philosophy is this revised view of the nature of language associated ? The key to the solution of our problems still lies in their source, *viz.* language itself. But we are

not to try to improve on, to tamper with, language ; only to describe its workings. For the confusions we are troubled by arise not when language is doing its work, but when it is idling, on holiday ; it is when we consider words and sentences in abstraction from their linguistic and non-linguistic contexts that they seem to conceal a mystery and invite a myth. So we are to " assemble reminders " of obvious facts about their uses ; not at random, nor yet systematically ; but on each occasion with some particular purpose in view, the purpose of dispelling some particular confusion. And to make clear the ways in which words actually function, it will sometimes be helpful to consider ways in which they do not, but might, function ; invented language-games will be useful objects of comparison with actual language-games (109-133).

Many philosophers would agree with much of this : it is difficult not to share the conception of philosophy held by the first philosopher of the age. Yet there are at least two very different directions in which it may seem unduly restrictive. First, there is the idea that the *sole* purpose of the distinctions we draw attention to, the descriptions we give of the different ways in which words function, is to dispel particular metaphysical confusions ; and, associated with this, an extreme aversion from a systematic exhibition of the logic of particular regions of language. Now, even if we *begin* with a therapeutic purpose, our interest might not exhaust itself when that purpose is achieved ; and there can be an investigation of the logic of sets of concepts, which starts with no purpose other than that of unravelling and ordering complexities for the sake of doing so. The desire to present the facts systematically here becomes important in proportion as therapeutic aims become secondary. The other direction might be suggested by what Wittgenstein himself says of certain metaphysical doctrines such as solipsism. The inventor of such a doctrine has discovered " a new way of looking at things "—something like " a new way of painting . . . or a new kind of song " (401). It is surely over-puritanical to hold that, just because the claims made for such new ways were too large, we should be concerned solely with preventing ourselves from seeing the world afresh. We might make room for a purged kind of metaphysics, with more modest and less disputable claims than the old. But one does not need to have an equal sympathy with both these possibilities in order to ask : could not the activities we call ' doing philosophy ' also form a family ?

Meaning and Understanding. (See first : 132-242, 319-326, 357-358, 431-436, 454. Further references are given later in this section.) Wittgenstein next reverts to the themes of *meaning* and *understanding* something by an expression—a theme announced, but not developed, at the end of the introductory paragraphs (33-36). The main sections which he devotes to this topic are of great brilliance and clarity. He begins with the point that the fundamental criteria for whether someone has understood an expression

lie in the application which he makes of it. Of course, he may correctly be said to ' grasp the meaning in a flash '. A picture or a paraphrase may come before his mind ; he may produce them to us. But neither picture nor paraphrase dictates the use that is to be made of it : they can be *variously* applied, and if we are inclined to forget this, it is because we are inclined to think of only one application. So the production of the picture or paraphrase, though normally (and rightly) enough to satisfy us, is not the final test : *that* resides in the application. Now this may seem acceptable enough so long as we are considering the criteria we employ for someone else's understanding. But do we not often and correctly say of *ourselves*, when, *e.g.* someone is trying to teach us how to develop a series, things like : ' Now I understand ! ', ' Now I can go on ! ' ? It is obvious that we are not here applying to ourselves the criterion of application : we seem to be reporting something which " makes its appearance in a moment ". No doubt there usually is some momentary experience : *e.g.* a formula for the series may occur to us, or we may just experience a feeling of release of tension. But, though in certain cases, a remark such as ' The formula has occurred to me ' might have the same force, serve the same purpose as ' Now I understand ' etc., it will not do to say that in general these expressions have the same meaning (183). In any case it is clear that *the understanding* is not to be identified with any such characteristic experience. (This will seem only too clear ; and is the point at which we are tempted to look for a special experience.) If we ask what, *in the eyes of others*, " justifies " me in using the words ' Now I understand ' etc., what shows my use of them to be " correct ", it is not the occurrence of the experience, whatever it may be, but the circumstances under which I have it (*e.g.* that I have worked with these formulae before, that I now continue the series, etc.) (153-155). This obviously does not mean that the words ' Now I can go on ! ' are short for a description of all these circumstances, or that they mean ' I have had an experience which I know empirically to lead to the continuation of the series ' (179) ; my certainty that I shall be able to go on is not a matter of induction (*cf.* 324-328). What we need (here I interpret a little) is to look at such first-person utterances in a radically different way from the way in which we look at the corresponding third-person utterances : to see them not as reports about myself for giving which *I* have to apply criteria, but rather as " exclamations " (323) or " signals " (180, p. 218), naturally and appropriately made or given in certain circumstances. Such an exclamation could even be compared with " an instinctive sound " or " a glad start " (323). Failure to perform successfully after the signal had been given would not necessarily mean that it had been incorrectly given. In some cases (*e.g.* if there had been an unforeseen interruption or disturbance) we should accept the plea : ' When I said I knew how to go on, I did know ' (323).

Two minor comments. (1) Wittgenstein does not enlarge on the suggestion given by the word ' signal '. One might take as a typical case that in which a teacher turns from the blackboard, proffers the chalk to the class with the question ' Who can go on ? '. Here the answer ' *I* can ' would have the same function as a silent acceptance of the chalk. (2) Wittgenstein's continual use of phrases like ' gave him the right to say ', ' made it a correct use of ', ' justified him in saying ' in connexion with these first-person utterances might tend to obscure a little his own doctrine. The essential point is that a person does *not* have (or need) grounds or reasons (does not apply criteria) for saying correctly that he himself understands, in the sense in which others must have them to say it of him. Of course, he may himself have, or lack, reasons or justifications in another (a social) sense.

Grasping the meaning (understanding it) should be compared with intending the meaning (meaning it). The two are connected in this way. We may be inclined to think that the purpose of the explanations we give (of, *e.g.* the rule for the development of the series) is really to get the learner to catch hold of something essential, the meaning we *intend* (210) ; and that the correct application he then makes of the rule is somehow a consequence of his catching hold of this essential thing. And here we have the idea of the mental act of *intending, meaning* this expression of the rule in a certain way, as somehow anticipating and predetermining all the steps of its application before they are taken. Or the connexion may be approached in another way. The criterion for the learner's having understood the rule aright is his application of it. But not *any* application (anything that could possibly be represented as an application of the expression of the rule) will do. It must be the *correct* application. But what are the criteria for correctness ? Here again one is inclined to answer that the correct application is the one that was meant : and this answer may once more give us the picture of all the steps being somehow covered in advance by the act of meaning. This idea is *very* compelling. For does not the *sense* of the rule (of the expression) determine what is to count as the correct application of it ? What does the determining here is not just the words or the symbols themselves. They are dead, inert (432, 454) ; they could be applied, and applied systematically, in indefinitely numerous different ways. And the same goes for any paraphrase or picture (433), the substitution of one expression or symbol for another : " interpretations (in this sense) by themselves do not determine meaning " (198). It is natural to suppose that only the intention, " the psychical thing ", can do that. Natural, but of course wrong ; (or at least misleading). The criterion for the correct application of the rule is *customary practice* (199-201) ; the customary practice of those who have received a certain training ; the way we are taught to use the rule and do always use it. It is, too, in the existence of this customary practice,

and nowhere else, that we may find the way in which the steps are "determined in advance", and the way in which a certain application is "meant", or "intended" by the ordinary instructor who has mastered that practice. (Of course; for 'the rule determines this application' means 'the rule is not correctly understood unless used in this way'.) What gives us the illusion of some other mysterious determinant is the fact that, having received a certain training, we find it so *utterly natural* to make a certain application of the expression. We draw the consequences of the rule "*as a matter of course*"; and cannot understand how anyone can make a wholly different application of the expression. ("'But surely you can see . . . ?' That is just the characteristic expression of someone who is under the compulsion of a rule" (231).) Of course, in the instructor-pupil situation, explanations are in place; but the purpose of the explanations is to get the pupil to do as we do, and find it equally natural. "Explanations come to an end somewhere"; then we just act. But equally, of course, "the pupil's capacity to learn may come to an end".

Among resistances to his views Wittgenstein notes the tendency to fall back on the words 'the same': the tendency, *e.g.* to say 'But when we explain a rule to someone by giving him examples of its application, all we want him to see is that he is *to do the same* in other cases'. In answer to this, Wittgenstein points out that the idea of a single, generally sufficient criterion of 'what is the same' is nonsense. The criteria for 'doing the same' in *the case of a particular rule* are just the criteria, whatever they may be, for the correct observance of that rule. The concepts of 'rule' and 'identity' are as closely related to each other as the word 'proposition' to the phrase 'true or false'. (See 185, 208, 215-216, 223-227.)

Now although Wittgenstein, in the sections here discussed, is directly concerned mainly with the topics of meaning or understanding something by an expression of a rule, what he says is taken by him to have much wider implications about language in general. These implications give the meaning-use equation a new significance, of the utmost importance in relation to the topics of experience and sensation which he next discusses. Roughly speaking: what he says about obeying what we would ordinarily call 'rules' is applied to obeying what philosophers are apt to call 'the rules for the use' of all expressions, whether or not they are the expressions of rules. Obeying a rule is conforming to an agreed common practice; "it is impossible to obey a rule 'privately'" (202). The emphasis is on the *agreed common practice* in the use of expressions, and this carries with it, in cases where it is appropriate to speak of criteria, the existence of *agreed common criteria* for their application. Wittgenstein notes that this in its turn demands the existence of general agreement in *judgments* (242). The great importance of these points emerges rapidly in what follows.

Before turning to Wittgenstein's discussion of his next topic, it is in place to notice some of the further things he says about ' meaning something by an expression '. To this he reverts at intervals, in a rich succession of examples, arguments and suggestions throughout the rest of the book (see especially 503-510, 525-534, 540-546, 592-598, 607, 661-693, II. ii, II. vi, II. xi, pp. 214-219). As before, the main doctrine attacked is the doctrine that what gives an expression, in use, its meaning and its life, is the user's special experience, or act, of meaning something by it. I select some principal points.

(*a*) One of the sources of the doctrine is the difference we feel between using words in an ordinary, and in an abnormal, context (*e.g.* repeating a sentence mockingly, or as a quotation, or in elocution practice, etc.). Aware of a difference of feeling in these latter cases, we are apt to suppose it consists in the absence of the normal experience of meaning what we say. Just so might we suppose, until we remember the facts, that everything that does not feel strange, feels familiar (592-598, 607 ; *cf.* also, on ' being influenced ', 169-170).

(*b*) In II. vi (*cf.* also 592), Wittgenstein analyses brilliantly the idea of a special atmosphere or feeling carried by each particular word. (So some philosophers have spoken, for example of an ' if-feeling '.) Whatever feeling-accompaniment the reading or uttering of a certain word may have, it is only *as so accompanying the word* that we are tempted to invest it with this special significance. Any such feeling or sensation might occur in a different context and not be recognised at all. But a feeling or atmosphere which loses its identity (its identity as, say, the ' if-feeling ') when separated from a certain object, is *not* a special feeling or atmosphere associated with that object at all. Contrast it with a genuine case of separately identifiable but closely associated things. Here a different association would shock and surprise us.

(*c*) Wittgenstein does not deny that a sense can be given to the notion of *experiencing* the meaning of a word. (See II. ii and II. xi, pp. 214-216. *Cf.* also 526-534.) This phrase might reasonably be used in connexion with many experiences which we do have, and which profoundly affect our *attitude* to language. Among these are : finding the *mot juste* after rejecting a number of candidates ; reading with *expression* ; saying a word again and again until it seems to ' lose its meaning '. In particular Wittgenstein notes the game of pronouncing a word like ' March ', now with one, now with another of its meanings. We can comply with the instruction to do this, and can perhaps report a different experience of the word. But this very game shows how unimportant the experience is in relation to meaning the word now in this way and now in that in the ordinary course of events ; for then we may not be able to report any such experience. In general, the word ' meaning ' may be said to acquire a secondary use in

connexion with all these experiences which are of such significance in relation to the way we *feel* about our language. But this use *is* secondary. Words could still have their meanings, language be used as a means of communication, in the absence of these phenomena.

(*d*) The most important of these later discussions (661-693, and pp. 216-217) is concerned with meaning in the sense it has in such a question as 'Whom (which one) did you mean?', *i.e.* with an intended reference as opposed to an intended sense. But there are close analogies between the two. When, with reference to an earlier remark, I say 'I meant him' I refer to a definite time (the time at which I made the remark); but not to an experience which I had at that time. There may sometimes be characteristic accompaniments, looks, movements of attention; but these are not what 'meaning him' consists in. Compare 'I meant him' with the phrase 'I was speaking of him'. What makes it true that I was speaking of him is the set of circumstances in which the original remark was made, and in particular, the general set or direction of my actions, of which the explanation ('I meant him') is itself one. What is apt to confuse us here is that of course I do not discover whom I meant by studying those circumstances. But nor do I report an indubitable special experience. It is rather (here I interpret a little) that in giving the explanation of whom I meant I continue a certain chain of actions; as I might, in pointing things out on a complex diagram, discard a blunt pointer in favour of a finer one, more serviceable for my purpose. Or I might *dis-continue* that chain of action, think better of it; and substitute another name; and then I shall be said to have lied. The question *not* to ask is: How do I know whom I meant? For I have no *way* of knowing, I apply no criteria. (Wittgenstein suggests that the words 'know' and 'doubt' are both out of place here. But of course, one can in fact be said to forget whom one was speaking of, and thus to be in doubt, to be 'not sure'; and then to remember. But this is an unimportant qualification; for Wittgenstein discusses elsewhere the question of *remembering* one's intentions.)

Pain and Persons. (See 142, 243-315, 350-351, 384, 390, 398-421, II. iv, II. v.) Studying the sections in which Wittgenstein deals with sensations, one may well feel one's capacity to learn coming to an end. Wittgenstein's case against saying that the phrases 'meaning/understanding something by an expression' stand for or name special experiences seems to me thoroughly made out. But even the significance of this denial comes into question if it then appears that no word whatever stands for or names a special experience. (Experiences which characteristically accompany 'I understand!' are not to be identified with understanding. Try substituting "'I am in pain'" and 'pain' in the appropriate places here.) Wittgenstein seems to me to oscillate in his discussion

of this subject between a stronger and a weaker thesis, of which the first is false and the second true. These may be described, rather than formulated, as follows. (I attach no importance to their descriptions: their significance must emerge later.) The stronger thesis says that no words name sensations (or 'private experiences'); and in particular the word 'pain' does not (*cf.* 293). The weaker thesis says that certain conditions must be satisfied for the existence of a common language in which sensations are ascribed to those who have them; and that certain confusions about sensations arise from the failure to appreciate this, and consequently to appreciate the way in which the language of sensations function. The oscillation between the two theses is to be explained by the fact that the weaker can be made to yield to the stronger by the addition of a certain premise about language, *viz.* that all there is to be said about the descriptive meaning of a word is said when it is indicated what *criteria* people can use for employing it or for deciding whether or not it is correctly employed.

The stronger thesis is first developed by an attack on the idea of a private language (243 *et seq.*). By a 'private language' we are here to understand a language of which the individual names (descriptive words) refer solely to the sensations of the user of the language. He may be imagined as keeping a record of the occurrence of certain sensations. The main point of the attack is that the hypothetical user of the language would have no check on, no criterion of, the *correctness* of his use of it. We might be inclined to say that his memory provides a check. But what check has he on his memory? Suppose in the case of one particular word he keeps on misremembering its use. What difference will it make? There will be no way in which he can distinguish between a correct and an incorrect use. So the idea of a correct use is empty here: and with it the idea of such a language. Now the interesting thing about the attack is that Wittgenstein presents it as if it applied only or peculiarly to the idea of a private language in which all the descriptive words were supposed to stand for sensations. But of course, if it has any validity at all, it has an equal validity for the case of a private language (here this means 'a language used by only one individual') in which the words stand not for sensations at all, but for things like colours or material objects or animals. Here again the individual will have no external check on the correctness of his use of the names. (It is no good saying that he can, in this case, though not in the case of the sensation-language, make himself a physical dictionary, *e.g.* a table with names opposite pictures. Wittgenstein's own arguments in other places are here decisive. The interpretation of the table depends on the use that is made of it.) But if this is so, then Wittgenstein's arguments would at most tend to show that the idea of a language *of any kind* used only by one person was an absurdity: and this con-

clusion would have no special immediate relevance to the case of sensation.

But it is clear that Wittgenstein intends his arguments to have special relevance to this case. So let us look at the differences between the two supposed private languages. We may suppose (following Wittgenstein) that in the case of the first private language, the one for sensations only, there are no particular characteristic overt expressions of the sensations, manifested by the hypothetical user of the language. Now we introduce observers, studying the behaviour and surroundings of the language users. The observer (B) of the user of the second language observes a correlation between the use of its words and sentences and the speaker's actions and environment. The observer of the user of the first language does not. Observer B is thus able to form hypotheses about the meanings (the regular use) of the words of his subject's language. He might in time come to be able to speak it: then the practice of each serves as a check on the correctness of the practice of the other. But shall we say that, before this fortunate result was achieved (before the use of the language becomes a *shared* "form of life"), the words of the language had no meaning, no use? And if we do not say this of the case where this result can be achieved, why should we say it of the hypothetical case where it cannot? In each case, while the language is used by one person alone (let us say as in one of Wittgenstein's examples, to record occurrences), the meaning of the words is a matter of the customary practice of the user: in each case the only check on this customary practice is memory. But (it might be said), the hypothesis that someone was using a language of the first kind could never be tested. But what is to count as a test here? Suppose he also mastered the ordinary common language, and then *told us* that he had been (or was still) using a private language. It is also *just* worth asking, in connexion with some of Wittgenstein's arguments here: Do we ever in fact find ourselves misremembering the use of very *simple* words of our common language, and having to correct ourselves by attention to others' use?—Wittgenstein gives himself considerable trouble over the question of how a man would *introduce* a name for a sensation into this private language. But we need imagine no special ceremony. He might simply be struck by the recurrence of a certain sensation and get into the habit of making a certain mark in a different place every time it occurred. The making of the marks would help to impress the occurrence on his memory. One can easily imagine this procedure being elaborated into a system of dating. (The purpose of these remarks is to indicate the place the sensation-name might play in the private language-game.)

Another of Wittgenstein's main arguments is basically a variant on the first. He notes two associated points, which one is not inclined to dispute: (1) "the expression of doubt has no place in the language-game" (288), *i.e.* in the language-game with 'I am in

pain ' ; [1] and (2) " what I do (when I say ' I am in pain ') is not to identify my sensation by criteria " (290). Wittgenstein seems to think that these facts can only be accommodated if we regard ' I am in pain ' as an *expression* or manifestation of pain, alongside such natural expressions as crying or groaning, but of course, one which, unlike these, is the result of training (244, 288, etc.). So regarded, ' pain ' ceases to appear as the name or the description of a sensation. If we do *not* so regard it, then we shall require criteria of identity for the sensation ; and with these there would enter the possibility of error (288). (Here one is, of course, reminded of the discussion of understanding. There are criteria of understanding, as of pain, which we apply in the case of others. But a man does not apply them to himself when he says ' I understand '—and his saying this is to be compared with an exclamation, a glad start, not a report of a mental occurrence.) Wittgenstein here seems to me to be in a muddle : the weaker thesis is being muddled with the stronger. What he has committed himself to is the view that one cannot sensibly be said to recognise or identify anything, unless one uses *criteria* ; and, as a consequence of this, that one cannot recognise or identify sensations. But of course this is untrue. Consider cases where the rival pull of ' expression ' or ' manifestation ' is weaker or non-existent. Consider, for example, tastes ; and such phrases as ' the taste of onions ', ' a metallic taste '. Here we have one thing (a taste) associated with another (a material substance), but quite certainly recognisable and identifiable in itself. Only, of course, one does not use *criteria* of identity for *the taste*. If the question ' What is the criterion of identity here ? ' is pushed, one can only answer : ' Well, the taste itself ' (*cf.* ' the sensation itself '). Of course, the phrases by which we *refer* to such tastes involve allusions to what can be seen and touched ; for we speak a common language. But we do not identify the taste by means of the associated substance by allusion to which we name it. Consider also that we discriminate and recognise different particular pains (I do not mean pains in different places)—aches and throbs, searing and jabbing pains, etc. In many cases, there are not, or not obviously, different characteristic natural expressions of pain to correspond to these differences in quality. The phrases by which we name these particular pain-experiences are commonly analogical ; and this too for the reason that we want a common language.[2] All the

[1] Even this is not quite true as it stands. There is no place for one kind of doubt, the kind that might be expressed in ' Am I interpreting the facts of this situation aright ? ' But there is sometimes place for another kind, which might be expressed in ' Does this quite deserve the *name* of " pain " ' ?

[2] The fact that there are characteristic natural expressions of pain but not (or not obviously) characteristic differences between natural expressions of different kinds of pain (I do not mean different degrees or locations) is the explanation of the fact that the descriptions of the different kinds tend to be analogical, whereas the word ' pain ' is not analogical. See below.

time here Wittgenstein is driving at the conditions that are necessary for a common language in which pain can be ascribed to persons, the consequent need for *criteria for the ascription* of pain, and the effects of this upon the use of the word ' pain ' of our common language. Hence his obsession with the *expression* of pain. But he errs through excess of zeal when this leads him to deny that sensations can be recognised and bear names. Rather the case is that these names must always contain in a more or less complex way, within their logic, some allusion to what is not sensation, to what can be seen and touched and heard.

Why is this ? The answer illuminates the nature of Wittgenstein's mistake, and also the great extent to which he is right.

(1) To deny that ' pain ' is the name of a (type of) sensation is comparable with denying that ' red ' is the name of a colour.

(2) It is just the difference between the ways colours and pains enter into our lives that accounts for the fact (i) that we call the latter and not the former sensations (or, alternatively, that accounts for the very special status we assign to sensations) ; it is just this difference that accounts for the fact (ii) that we *ascribe* pains to those who suffer them and not colours to those who see them ; and which accounts for the fact (iii) that *without* criteria for ascribing pains to persons, we could have no common language of pain. Finally, it is because our common language-game must be that of ascribing pains to persons that symptoms, expressions, of pain assume such overwhelming importance.

(3) Fact (iii), misunderstood, is reflected (upside down) in all the usual philosophers' confusions about sensations ; and, over emphasised, is reflected (rightside up, but obscuring the rest of the picture) in Wittgenstein's.

To understand at least part of what is meant by ' the difference between the ways colours and pains enter our lives ', it will be helpful to consider some unrealised possibilities. Let us suppose first that we feel pain only and always under the condition that our skin is in contact with the surfaces of certain bodies. (*Cf.* Wittgenstein's ' pain-patches ', 312. But he does not exploit this fully.) The pain begins and ends with the contact. Then our pain-language might have a logic wholly different from that which it does have. Instead of ascribing pains to sufferers, we might ascribe painfulness to surfaces, much as we at present call them rough, smooth, hard, soft, etc. Another possibility is this. We say things like ' It's hot in here ', ' It's cold out there ', and so on, ascribing temperatures (I do not mean in degrees Fahrenheit or Centigrade) to regions. Let us suppose that any person felt pain if and only if every other normal person in the same region (which could be the size of a room or a continent) at the same time also felt pain. Then we might ascribe painfulness to regions instead of pain to persons ; saying, *e.g.* ' It's painful today ', or ' It's painful in here '. The point of both examples is that in each case we

should have as *impersonal* a way of describing pain-phenomena as we have of describing colour-phenomena. But of course the incidence of physical pain is not like this. The causes of pain are often internal and organic. Even when pain is caused by contact, it generally requires a special kind of contact rather than contact with any special kind of thing; and it generally does not cease when contact ceases. If you have a pain and I come to the place where you are, or touch or look at what you are touching or looking at, this will not in general result in my having a pain. As Wittgenstein not infrequently points out, it is such very obvious general facts of nature which determine the logic of our concepts. I may put the point very roughly as follows. A set of people together in certain surroundings will be in general agreement on ' what it looks like here, what it feels like (to the touch) here, what it sounds like here '. In this possibility of a general agreement in judgments lies the possibility of a common impersonal language for describing what we see and hear and touch (*cf.* 242). But there is no such general agreement as to whether or not ' it's painful here ', as to what it feels like (as we misleadingly say) *within*. In the absence of general agreement in judgment, a common language is impossible; and this is why a common impersonal pain-language is impossible. But if (to speak absurdly) we are prepared to make our pain-language a language for ascribing pain to persons, then we have something (*i.e.* people's pain-behaviour) which we see and hear, and on which, in consequence, general agreement in judgment is possible. Because of certain general facts of nature, therefore, the only possible common pain-language is the language in which pain is ascribed to those who talk the language, the criteria for its ascription being (mainly) pain-behaviour. And because of *this* fact it is necessarily empty and pointless (I will *not* say meaningless) either (*a*) to speculate about the ascription of pain to anything which does not exhibit behaviour comparable in the relevant respects with human behaviour (*i.e.* the behaviour of those who use the concept), or (*b*) to raise generalised doubts about other people's experience of pain, or about one's own knowledge of this. It is the above points which I take Wittgenstein essentially to be making. (He sees (142) that, as things are, the possibility of the language-game rests on there being characteristic expressions of pain.) But the way in which he makes them is, in part at least, misleading. For from none of these facts does it follow that ' pain ' is not the name of a sensation. On the contrary. It is only in the light of the fact that ' pain ' is the name of a sensation that *these* facts are intelligible; or, better, to say that ' pain ' is the name of a sensation is (or ought to be) just to begin to draw attention to these facts. One could say: that pain is a sensation (or, that sensations have the special status they have) is a *fact of nature* which dictates the logic of ' pain '.

This outline is of course extremely crude. What a proper treatment of the topic above all requires is extensive and detailed

comparisons between the different types of sensible experience we have.

Thoughts and Words. (316-394, 427, 501, 540, 633-637, II. xi, pp. 211, 216-223.) Wittgenstein's treatment of this topic has close connexions with his account of ' meaning/understanding something by an expression ' ; and presents certain analogies with the account of pain. It should first be noted that Wittgenstein is not primarily concerned with certain rather specialised applications of the word ' thinking ' which are apt to come first to mind. We sometimes contrast the thinker and the active man ; or may speak of having spent a fortnight's holiday without having a single thought. But Wittgenstein is not especially concerned with the thoughts which come only to the reflective. Again, we sometimes use ' thinking ' in the sense of ' thinking out how to do something ', or ' solving, or trying to solve, a problem ', when the problem may indeed be as practical a one as you please. But Wittgenstein is not especially concerned with the thinking that overcomes difficulties. His concern is not restricted in either of these ways. He is quite as much interested in the most ordinary case of ' having a thought ' or ' thinking something ', in the sense in which, for example, someone who, talking neither at random nor insincerely, says that p, may be said to have had the thought that p, or to think that p, or to have said what he thought. The close connexion between Wittgenstein's discussions of ' meaning something ' and ' thinking ' should now be clear. For if a man satisfies the criteria for meaning a sentence in a certain way (and is not insincere in uttering it), then he also satisfies the criteria for having thought something and said what he thought. So much of the argument on the former topic bears directly on the latter.

There is a certain once common view of the nature of thinking against which Wittgenstein's arguments are mainly directed. It is this. Thinking or having a thought is a special event or process which may accompany and be expressed in speech or writing or relevant action, and may also occur in the absence of these. No one can ever know what another's thoughts are in just the way he knows what his own are ; for each man is directly cognisant only of his own internal processes. Wittgenstein's general counter-thesis runs as follows. It is true that having a thought is not to be identified with any particular outward process of speech or writing or action ; nor with inner speech or other imagery. The having of a thought is not the occurrence of any of these ; but nor is it any other occurrence. It is the occurrence of one of these in a certain context, in certain circumstances. To see what sort of context, what sort of circumstances are relevant here, one must consider what criteria are used in ascribing thoughts to people. A man's actions may show his thought (*cf.* 330), or his remarks may tell it ; but if a monkey imitated the actions, or a parrot repeated the words, we should not ascribe the same thought to the monkey or the

parrot. The difference does not lie in what went on inside them, but in the difference between the rest of their behaviour. Of course, a man does not, in telling his own thoughts, apply to himself the criteria for ascription of thoughts which he applies to others. But neither, when he says what he is or has been thinking, does he report on any concurrent or antecedent inner process. It is rather that he takes, or continues, a certain line of linguistic action, much as he might take or continue, in other circumstances, a certain line of non-linguistic action ; and of course, he might ' think better of it ', *change* his line.

It is quite clear and very important that Wittgenstein does not deny the existence of those occurrences, whether observable, like an exclamation, or unobservable, like what we call a flash of insight, or inward speech, by reference to which we may *date* the occurrence of a thought. What he is most concerned to stress is the fact that these occurrences do not owe their significance or their claim to the titles they bear, to their own peculiar nature or that of some psychical accompaniment ; but to their place in a general pattern of actions and events. The concept of thinking demands such a general pattern as a setting for the occurrence of thoughts. In this, which I shall call his hostility to the doctrine of immediacy, Wittgenstein is surely right. Another factor in his treatment of thinking which is, I think, quite distinct from this, and also more questionable, might be called his hostility to the doctrine of privacy. It is because of this factor that I speak of analogies with his treatment of pain. I shall try to illustrate this from some of the things he says about inward speech, *i.e.* about saying something to oneself in imagination. (I would emphasise that the hostility to the doctrine of privacy is not of peculiar importance in connexion with *thinking*. It has more general application. It is merely that it can be very clearly illustrated by some things that Wittgenstein says in the course of his discussion of thinking.)

II. xi, pp. 220-223 are largely concerned with telling or confessing what one ' has been saying to oneself in one's thoughts '. Wittgenstein says : (1) that what I say to myself in my thoughts is hidden from others only in the sense in which my thoughts are hidden from one who does not understand the language in which I speak out loud (pp. 220, 222) ; and (2) that when I tell another what I was saying to myself, or acknowledge that he has guessed aright, I do not describe what went on inside me : I do not tell what I was thinking from inspection of the inner process (p. 222). Now it seems to me that Wittgenstein is here equivocating with ' what I say to myself ' ; and that his motive for doing so is his hostility to the idea of what is not observed (seen, heard, smelt, touched, tasted), and in particular to the idea that what is not observed can in any sense be recognised or described or reported. The equivocation is made possible by the fact that ' what I was saying to myself in my thoughts ' can mean either ' what I was thinking ' or

'what words were going through my mind'. Now it is true that in saying what I was thinking, I do not report on 'what went on within me' (p. 222). In just the same way I do not tell what I am imagining by inspecting my image (II, iii). But also in just the same way I do not, in the case where I am talking or thinking aloud, discover what I am or have been thinking by listening to my words or their echo. As far as this goes, audible and inner speech are on the same level. (This fact indicates both the respect in which remark (1) above is justified and also the respect in which is false.) They are on the same level in another way too. I may tell what I was saying to myself, not in the sense of telling that I was thinking, but in the sense of reporting what words were going through my mind. And here 'what went on within me' is just as much to the point, as 'what went on audibly' is when I am asked not to say what I meant when I spoke aloud, but to *repeat my words*. Of course, the difference between *these* two cases is that there is, and can be, no check (except the general reliability of my short-term memory) on my report of what words were going through my mind, whereas there can very well be a check on the correctness of my repetition of my audibly spoken words. But only a prejudice against 'the inner' would lead anyone, on the strength of this difference, to deny the possibility of the first kind of report. Only the same prejudice would lead anyone to deny that I can sometimes say something by way of description of my experiences of having imagery, as well as describing to people what I am imagining. (Having a confused and jumpy or intermittent image is not the same as imagining something as confused and jumpy or intermittent.) It is very likely true that some have not distinguished describing or reporting such experiences from telling what one is thinking, or describing what one is imagining. That a fact can be misconstrued is not, however, a reason for denying it. It is also true that when we describe 'private' or 'inner' or 'hidden' experiences, our descriptions of them (like our descriptions of their status) are often *analogical*; and the analogies are provided by what we *do* observe (*i.e.* hear, see, touch, etc.). This is in itself an important fact. It throws light once more on the conditions necessary for a common language. One could almost say that it is this fact which Wittgenstein is often stressing, often in a perverse way. But a description is none the worse for being analogical, especially if it couldn't be anything else.[1] Moreover, some of the analogies are *very good ones*. In particular the analogy between saying certain words to oneself and saying them out loud is very good. (One can even be unsure whether one has said them out loud or to oneself.) The analogy between mental pictures and pictures is, in familiar ways, less good.

[1] If one were *very* anxious to appease here, one *could* say they were only 'descriptions' in an analogical sense. But I think this would be being over-anxious.

Perhaps what is really operating here is the old verificationist horror of a claim that cannot be checked. Elsewhere in his treatment of thinking, Wittgenstein manifests the more intelligible aversion from a hypothesis or supposition that cannot be checked (344, 348-349).

States of Mind and Introspection. (572-587, II. i, ix, x; also 437, 465.) Wittgenstein writes on expectation, hope, belief, wishes, grief and fear. As in other cases, the main hostility is to the doctrine of immediacy. Expectation, hope, grief are forms of human life, each with many variations. What the subject of these states is at any given moment doing or experiencing, gets its significance, its importance, from its surroundings (583-584), its context of situation and behaviour. The isolated occurrences could not claim these names. The falsity of the doctrine of immediacy is indeed, in some of these cases, a great deal more evident than it is in the cases of thinking or understanding. For a man may be said to expect or to believe or to be grieving over something when no thought of that thing is in his mind. There is therefore an *a fortiori* character about the non-immediacy of the states that these words name.

Among the criteria that we use for the ascription of certain of these states to another person, the *verbal* behaviour of the subject of them assumes an overwhelming importance. Wittgenstein in fact suggests that a necessary condition of the ascription of, *e.g.* hopes, wishes and some beliefs to a subject is the subject's mastery of a language for the *expression* of these (see II. i, ix and 650). His reason for this is clear. A creature's non-linguistic behaviour may certainly provide adequate criteria for the ascription of *some* states; but where descriptions of states take on a slightly higher degree of complexity, it may be difficult or impossible to imagine what sort of complication of non-linguistic criteria would be adequate in the case of beings incapable of speech. ("We say a dog is afraid his master will beat him; but not, he is afraid his master will beat him to-morrow.") In the sections on thinking Wittgenstein placed an analogous and obviously connected restriction on the ascription of thoughts. It seems that one ought here to distinguish between the thesis that there are certain *kinds* of state (say, wishing or hoping) which cannot even in their simplest forms be ascribed to beings without the power of linguistically expressing them; and the weaker thesis that there are more complex forms of certain kinds of states (wishing, believing, hoping) which cannot be ascribed to beings without this power. The weaker thesis seems to me obviously true. Still more obviously, the stronger thesis must, on *one* interpretation, be false. Words like 'wish' and 'hope' could never acquire a use at all unless there were *some* circumstances or range of circumstances, other than that of their being uttered, in and because of which it was correct to use them; and since wishes and hopes are ascribed to others, these circumstances must include

criteria, must include the observable. Wittgenstein's general principles here refute his particular thesis. I may, however, very likely be mistaken in ascribing the stronger thesis in this form to Wittgenstein. His position may rather be the intermediate one that *some* linguistic capacities are required in the subject to whom wishes and hopes are ascribed, not the specific linguistic capacity for the conventional expressions of wishes and hopes. So amended, the doctrine cannot, of course, be refuted in this way.

What in general of the roles of first-person utterances about states of mind? Wittgenstein here draws an admirable distinction between those first-person utterances which are correctly called reports of the results of introspection, or descriptions of states of mind, on the one hand, and those which are only misleadingly so called (and only so called by philosophers), on the other. We can *come to conclusions* about our own hopes, fears, expectations, even beliefs; and in doing so we use much the same criteria as we use in coming to conclusions about others, though we may (or may not) have certain advantages in our own case. But first-person utterances about states of mind are more commonly not of this kind, but of another: not conclusions about, but conventional expressions of, states of mind, taking their (special) place among the other criteria which are used in ascribing them. I think Wittgenstein does not perhaps give quite enough weight to the *very* special nature of this place, that he tends to exaggerate a little the degree to which, or the frequency with which, these utterances are, so to speak, forced from us. We use them very often pretty deliberately, and to inform; to show others where we stand, what we may be expected to do and for what reasons. And this may safely be admitted—without either returning to the doctrine of reported special experiences, or advancing to the absurdity that *all* such utterances are *conclusions* about ourselves—just so long as we acknowledge, what Wittgenstein elsewhere examines, the nature of our certainties about own deliberate behaviour. The utterance of 'I am embarrassed/amazed/shocked/confident/very glad', etc. may be a social *act* (by this I mean something comparable with a polite greeting, a move to help someone, an offer of resistance, etc.); it may be a piece of deliberate self-revelation (compare exaggerating a *natural* facial expression) or an explanation of one's behaviour; it may be simply an embarrassed (amazed, etc.) *response*; it may be a conclusion about oneself, based on introspection (and only in this case does the question how one knows, arise); or it may be some, though not *any*, combination of these. I am not suggesting that Wittgenstein would dispute this diversity of function. On the contrary, much that he says (*cf.* especially II. ix on 'I am afraid') tends to emphasise it. It is rather that in his anxiety to stress the difference between most uses of these sentences on the one hand, and descriptions based on observation on the other, he tends perhaps to minimise their aspect as *deliberate exhibitions* of states of mind.

Immediately after the discussion of ' I am afraid ', Wittgenstein reverts briefly to ' I am in pain ' (p. 198). These words " may be a cry of complaint and may be something else ". It looks as if he were almost prepared to acknowledge here that they may be just a report of my sensations. Pain is not, like grief, a pattern of life. And a report can *also* be a complaint ; or a request.

Voluntary Action and Intention. (611-660, II. viii, II. xi, pp. 223-224.) What Wittgenstein says on these difficult topics is immensely suggestive and interesting—but elusive and incomplete. He begins with a brilliant short account (611-620) of our temptation, under the pressure of such a question as ' How do you raise your arm ? ' and of obvious analogies, to think of willing as a special act of which the phenomenal features of ordinary actions are consequences ; a very special act, for the obvious difficulties of this model then force us to think of it not as an act which we perform, but as one which just occurs, of the will as " only a mover, not a moved ". Then, abruptly, the problem is stated : " what is left over if I subtract the fact that my arm goes up from the fact that I raise my arm ? " This is sunk in the other question : " How do you know (when your eyes are shut) that you have raised your arm ? " The answer is " I feel it ", and this answer is correct, but misleading. For it suggests that you recognise the special feelings (the kinaesthetic sensations) *and can tell from recognising them* that their constant accompaniment, the raising of the arm, has occurred. And this is wrong. In fact, the certainty that you have raised your arm is itself a criterion of recognising the feeling here (625). It is important to note two things that Wittgenstein is *not* saying. He is not saying that one would have this certainty in the absence of any feeling, or in the presence of an unaccustomed feeling. It is no doubt *because* of the kinaesthetic sensations that I know ; but it is not *from* them that I tell (II. viii). Nor is he saying that I could never (say, if suitably stimulated) be mistaken about this (624). Here is a case where I know, but don't have a *way* of knowing, of telling. Now he reverts to the former question, and suggests : " voluntary movement is marked by absence of surprise " (628). As a *sufficient* condition (he neither says nor denies that he means it as one), this will not do. Experience of involuntary movements in certain circumstances might lead me to be quite unsurprised by their occurrence. The answer suggested by his own remarks about knowing, may be more useful. Voluntary movements are characterised by a certainty of their having been made, which neither has *nor needs* a ground ; though it may both have and, in another sense, need a cause. It is still far from clear that we have a sufficient condition ; for this seems true of many compelled or involuntary movements of parts of the body. But for the purposes of Wittgenstein's enquiry, I do not think a sufficient condition is required. For his purpose, I think, is to throw light from ' knowing what one has done ' on ' knowing what one is going to do

(intends)' and on 'knowing what one *was* going to do (intended)'. For there is no more reason to suppose that we need a *way* of knowing, of telling, in these cases than in that one. Of course, an announcement of a present intention, or a recalling of a past unexecuted intention is helped by, arises naturally from, the situation "in which it is (or was) embedded". But we do not read off or infer our intentions from these situations, any more than we tell what limb-movements we have just made from recognition of the accompanying sensations. (Nor in announcing or recalling our intentions do we report a current or remembered special experience. Look for this and it vanishes (645-6).) The point is that knowing what we intend to do is no more mysterious than knowing what we are going to do. (We as often make announcements of intention in the form 'I shall . . .' as in the form 'I intend to . . .'.) And a man may very well know what he is going to do (*be able to say*, if he is asked or if it should otherwise become desirable), and do it, without having raised the question to himself at all, without indeed having thought about it. The purpose—or one purpose—of announcing intentions is obvious: others have an interest in knowing what we will do. Remembering and telling a past unfulfilled intention is a less obvious case. An intention might be unfulfilled because I abandoned it or was interrupted or forestalled, etc.; or because my action failed of its effect. In no such case am I reporting something else that happened at the time. (This is particularly clear where my telling could be described as 'telling what I nearly did'.) It is rather that I exhibit *now* a response to the past situation; and my purpose in doing this may be to reveal to my auditor "something of *myself*, which goes beyond what happened at the time" (659).

Evidently this topic—of doing and intending—could be pursued into refinements and elaborations which Wittgenstein, with his fixed polemical purpose, neglects. But rarely has a subject been treated so powerfully and suggestively in so few pages.

Seeing and Seeing as. (II. xi, pp. 193-214.) By means of a series of examples and comments, Wittgenstein seeks to bring out some of the complexities of the concept of seeing. The cases he mostly considers are very special ones; and it is consequently difficult to see just how far his conclusions reach. One thing at least which gives under the strain of his examples is the doctrine of the purely sensory given on the one hand, and our interpretation of it on the other, as two ever-present but distinguishable elements in visual perception.

The examples of which Wittgenstein writes are examples of "the 'dawning of' (noticing of) an aspect". Sometimes when we look at a thing, we suddenly *see* it *as* something different from what we saw it as before, while also seeing that it has not in any way changed. Easily the best cases of this are provided by certain schematic pictures or diagrams; but we may also, *e.g.* suddenly recognise a face, or suddenly see its likeness to another. In none

of these cases is it to be supposed that we ' get a better (or different) view '. These cases, then, are to be sharply distinguished from those in which we suddenly see something because, say, a light has been switched on, or a screen removed. They are also to be distinguished from cases in which a man is able in certain ways to *treat*, say, a figure as a such-and-such or as a picture of a so-and-so, but does not have the experience of its suddenly assuming for him the aspect of a such-and-such or of a picture of a so-and-so. The difference from the first of these contrasted cases is that when a man *sees* something *as* x and then *as* y, there is a perfectly good sense in which it looks the same to him as it did before : *viz.* that drawings showing it as it looked to him before and after the change of aspect would be indistinguishable. The difference from the second contrasted case is that a man on whom an aspect dawns is not merely prepared to make different applications of what he sees, but also sees it differently, has the ' visual experience ' of a change of aspect.

Wittgenstein notes an ambiguity in ' seeing as ', which it is important to mention since it emphasises the special character of the experience he is concerned with. Suppose we have a visually ambiguous object such as Wittgenstein's duck-rabbit—a picture which can be seen as a picture of a duck or again as a picture of a rabbit. Then a man who never has the experience of the change of aspects, the dawning of an aspect, may nevertheless be said by those who know of its visual ambiguity to see it as, say, (a picture of) a rabbit. So a person may in an unimportant sense see something as something without having the experience in which Wittgenstein is interested. In neither sense is something which is not visually ambiguous—say, a conventional picture of a lion—normally said to be ' seen as ' anything.

Why is the experience of the dawning of an aspect of such particular interest to Wittgenstein ? He suggests that its importance resides in an analogy with ' experiencing the meaning of a word ' (p. 214). Perhaps we might say that when an aspect of a figure dawns on us, we are ' experiencing an interpretation of the figure '. What we have here is something instantaneous, a visual experience ; but it is also true that a *logical* condition of our having the experience is our being capable of making such-and-such applications of the figure, of reacting to it in certain ways, of *treating* it as what we *see* it as (p. 208). If we are to describe the experience correctly, we cannot isolate a ' pure visual element ' in it and say that that and that alone is the momentary experience (*cf.* p. 193) ; we can describe the experience correctly only by referring to what does not relate to the moment. (Wittgenstein is helped in making this point by the fact that the figure in one sense presents exactly the same visual appearance—does not look different—before and after the dawning of the aspect.) We have here, Wittgenstein says, a modified concept of experience, of seeing.

What Wittgenstein is essentially opposed to is the conjunction of the three propositions: (1) that we have here (*a*) a pure sensory element, and (*b*) an interpretation (a tendency to treat what we see in certain ways); (2) that (*a*) and (*b*) are simply associated or conjoined; and (3) that (*a*) alone is the visual experience proper. What may at first seem odd is not this opposition, nor the remark that we have here a modified concept of experience (contrast for example with pains), but the remark that we have here a modified concept of seeing (209). For surely ordinary instantaneous visual experiences, such as we have when the light is switched on and we suddenly see the room and its contents, can also be correctly described only by a description which entails our possession of concepts of certain kinds, and which thus refers beyond the moment. So in what way is the concept of seeing *modified* in the special case of seeing as? I think the *difference* between the cases, which leads Wittgenstein to speak of a modification of the concept, is the presence in the case where the light is switched on of just such an instantaneous change in the way things look, in the visual appearance of things, as is absent from the case where the aspect changes. But if this is right, it is not very happy to speak of a modification of the concept of seeing in a way which suggests that a certain feature which is common to both seeing and 'seeing as' is peculiar to the latter.

Conclusion. Wittgenstein has penetrating and illuminating things to say on other subjects: for example, identity and difference of meaning, meaninglessness, negation, induction, dreams and memory. But the topics which I have selected are those which receive most extensive treatment. On these I have tried to summarise and criticise his main arguments and conclusions, aware that much of the power, vividness and subtlety with which, by means of example, comment and epigram, these conclusions are presented, must in this way be lost. For this there is no remedy but study of the book itself.

Three cardinal elements in his thought, as presented in this book, may perhaps be epitomised in these quotations:

(1) "To imagine a language is to imagine a form of life" (19) and "What has to be accepted, the given, is—so one could say—forms of life" (p. 226).
(2) "What is happening now has significance—in these surroundings. The surroundings give it its importance" (583).
(3) "An 'inner process' stands in need of outward criteria" (580).

The first may serve to remind us of a general prescription for doing philosophy: to understand a concept, a word, put the word in its linguistic context and the whole utterance in its social context and then describe, without preconceptions, what you find; remembering that each word, each utterance, may figure in *many* contexts.

The second epitomises what I earlier called the hostility to the doctrine of immediacy. It has analogies with the first. Just as a word gets its significance from the context of its use, so those elements of our experience which we are tempted to isolate (or, failing this, to fabricate) and make the self-sufficient bearers of certain names get their significance too from their setting, from the form of life to which their titles allude.

The third, though it contains much that is true, contains also the germ of mistakes. It epitomises the hostility to the doctrine of privacy. For the worn and dangerous 'outward' and 'inner' we may substitute 'shared' and 'unshared'. Then what is right and wrong in Wittgenstein here begins from the insight that a common language for describing and reporting requires general agreement in judgments. So for a (descriptive) word or phrase to belong to a common language, it is essential that *the occasions on which it is right to apply it should provide shared experiences of a certain kind*, the existence of which is connected with the rightness of applying the word. The experiences need not be connected with the rightness of applying the word or phrase as *criteria* for its application. No one has *criteria* for something's looking red, though this is something on which we commonly agree. (Wittgenstein is perhaps misled here by temporarily confusing criteria which justify one in applying a word or phrase with criteria which justify one in saying that it is correctly applied. That there is a distinction is clear from much else that Wittgenstein says. For *every* expression of a common language (I speak here of non-analogical uses) there must exist, in the common practice of the use of the expression, criteria for determining whether a given use of it is correct or not. This will be so even where the expression in question has no descriptive use at all. So the existence of criteria for the correctness of the use of an expression does not entail that using it correctly is applying it on the strength of certain criteria. Wittgenstein would agree that the entailment does not *generally* hold; and there is no reason for thinking that it holds in the *special* case of descriptive or reporting uses of expressions. Moreover it could be independently argued that it *could* not hold here.) Now, when a word is not applied on the strength of shareable experiences, Wittgenstein is inclined to say that it is not applied in the way of report or description at all, but is used in some other way, is something else: *e.g.* a response, the habit of which is acquired by training, an action, a signal and so on. Sometimes, often, this is right: it is right in just those cases where the relevant shareable experiences count as criteria in the full, *logical* sense of that word. But there are other cases for which, taken

[1] Of course (*a*) the antithesis 'shared' and 'unshared' which I have used here, is a shorthand no more immune from misunderstanding than any other; and (*b*) I do not suggest that it represents a sharp division: tastes, for example, might count as an intermediate case, since one has to do something rather special to get them shared on a given occasion.

as a general rule, it is wrong. For sometimes a person applies a word or phrase not on the strength of shareable experiences but on the strength of non-shareable experiences, and *is* publicly reporting or describing those experiences; and this he is enabled to do either by the existence of shared experiences which count as signs (criteria in the weaker sense) of the occurrence of the unshared experiences (the case of 'I am in pain'), or by the adoption or invention of analogical modes of description, where the analogy is with shareable experiences (*e.g.* reporting the words that pass through my mind).[1] What misleads Wittgenstein here is, I think, partly the belief that criteria are always essential to a report or description, a belief in its turn based, perhaps, on the confusion, mentioned above, between criteria for application and criteria for correctness; and partly the fear of legitimising certain metaphysical doubtings and wonderings. As for these, they are, if you like, senseless: but in their own way, and not in any other. They are pointless and unreal and you can do nothing with them (unless you strip them of their form and use them to point a contrast); and that is condemnation enough.

Right or wrong, Wittgenstein's particular doctrines are of the greatest interest and importance. But the value of the book as a model of philosophical method is greater still. (Here I do *not* refer to idiosyncracies of style and form.) It will consolidate the philosophica lrevolution for which, more than anyone else, its author was responsible.

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VII.—NEW BOOKS

Biology and Language (Tarner Lectures, 1949-50). By J. H. Woodger. Cambridge University Press, 1952. Pp. xiv + 364. Price 40s.

Books such as this one, which are designed for readers of two rather different kinds, run the risk of being ignored by both, and it is up to a reviewer to show cause why this should not happen. What is there, then, in Professor Woodger's 'introduction to the methodology of the biological sciences including medicine', for readers of Mind?

The principal answer is: "A good example". It is often said that recent developments in logic and philosophical method are applicable to other than narrowly philosophical questions, but philosophers too rarely make these wider applications. This is just what Professor Woodger has always made it his task to do, and in the present book he aims to show how a number of questions on the borders of biology, psychology and philosophy can be settled by the use of suitable logical and linguistic tools.

The result is an absorbing, though rather heterogeneous series of lectures. Since modern analytical philosophy or, as he calls it, 'the Boole-Frege movement' means to Professor Woodger chiefly the American school (*e.g.* Quine, Tarski, Carnap, and Hempel), he spends the first part of the book (Lectures I and II) in expounding the basic terms and notions of symbolic logic, and the truth-functional analysis of general statements—which he takes as his model for theoretical statements. This is done in preparation for his second part (Lectures III to V) in which the fundamentals of genetical theory are to be set out in a completely formal and explicit way: this programme, he hopes, will enable him "to eliminate an enormous amount of unnecessary verbiage and to avoid the pitfalls to which the use of a natural language exposes us" (p. 43).

Much of the second part is technical and elaborately symbolical, but non-scientific readers should not be put off: if they turn to Lecture V they can take up the argument again. Professor Woodger's aim in this part is to do for genetics very much what Hertz did for dynamics in his *Principles of Mechanics*, and in due course he makes the same discovery as Hertz, namely, that the focal points of philosophical confusion in genetical theory are those at which phrases and terms with extraneous uses intrude—*e.g.* 'inherit', 'transmission of characters', '. . . determined by . . .'. The discussion of these confusions is excellent, though some may feel that Professor Woodger does not draw the right conclusion from his discovery. Surely the source of the trouble lies, not in a lack of definiteness in natural languages, but in the plain ambiguity of the terms cited: Woodger manages to unravel all the really serious confusions in clear English, so his symbolical apparatus might for these purposes have been dispensed with—thus avoiding the 'unnecessary verbiage' he speaks of without any corresponding increase in symbolage. However, it is important not to prejudge an issue which can only be determined in the practical field: it will be for geneticists themselves to decide in the light of experience whether the benefits of Woodger's symbolism are worth the price.

One may also wonder whether Woodger does not at times protest too much. For instance, in his analysis of genetics he stresses that all his

terms are defined as 'class-names' or as functors applying to classes, and he frequently complains about the slipshod use of the term 'properties' as applied to genes: yet if genetics had always been kept as antiseptic as he asks, would this not have slowed down its progress? It is difficult to see how, in a genetics formulated entirely in terms of 'classes of organisms' and their 'shared names', the role of the germ-cells could have come to be understood as quickly as it in fact was (*cf.* p. 25). This is not just to say that a little mental confusion may act as a healthy catalyst in the process of discovery, for Woodger is clear that his critical activity is a *post mortem* affair and is not designed as a method of discovery. The trouble is that his campaign against reliance on metaphors in science seems at times to be directed against the use of explanatory models also; and such a policy would succeed not only in purifying scientific theory, but in sterilising it as well.

In the last two lectures Professor Woodger turns to the mind-body problem, or rather, to what is more profitable, the relations between psychological and neurological language and explanations. Here his symbolic apparatus is no longer conspicuous, and the argument gains in cogency as a result. His observations in this part are not profound, but they are interesting and sensible: thus, "people are not always content when they have found a good thing. They want to believe that it is the *only* good thing" (of those who will accept only explanations in physical terms, pp. 278-279), "It has not been made at all clear why it should be a scientific sin to be anthropomorphic when we are dealing with *ἄνθρωπος* himself" (p. 283), "Words like 'psycho-somatic' are only invented to heal a breach which need never have been made" (p. 317), and so on. There is an intriguing polemic against the unhappy effects which Woodger believes Cartesian dualism has had on medical education and clinical procedures (pp. 294 ff.), and a useful discussion of Suttie's *Origins of Love and Hate* (pp. 301 ff.), which he holds out as a satisfactory example of a theory expressed in terms of persons. Much of this last part will be of interest to readers of MIND, particularly Professor Woodger's analysis of quotations from eminent biologists perplexed by such questions as 'where in the brain the mind intervenes'.

There are also three appendices, the most valuable of which is a full analysis of Harvey's *De Motu Cordis* (pp. 75-92), showing clearly the logical interrelations and interdependencies between the evidence appealed to and the theoretical propositions Harvey wishes to establish. Incidentally, it is worth noticing that Woodger sets out Harvey's argument quite succinctly without resort to any symbolism.

Two brief philosophical comments. (*a*) Professor Woodger is very hostile towards 'abstract entities', and follows Quine in all his attempts to smoke them out of logic and science; yet he takes as one of his starting-points a body of Russellian epistemology, in terms of 'sensible objects' called 'looks', 'feels', etc., which ought to offend him for just this reason. Perhaps he has not noticed that, when treated not as verbs but as *nouns*, the words 'look' and 'feel' become quite as 'abstract' as the property-words to which he takes exception ('tallness', etc.). (*b*) Boole and Frege both threw large rocks into the philosophical pond, and the ripples they caused can still be felt in many directions. There is certainly not any single 'Boole-Frege movement', nor any one true Boole-Frege gospel, and things are better so. It is a pity, therefore, to write as though the symbolic logicians alone were the legitimate heirs of

Frege, and as though contemporary philosophical method could be applied only to a science that has first been shaped to fit the truth-functional mould. This is especially unfortunate considering how many of Professor Woodger's most illuminating contributions to the philosophy of science, from the *Biological Principles* to the present book, have been presented without the use of logical hooks-and-eyes.

STEPHEN TOULMIN.

Translations from the Philosophical Writings of Gottlob Frege. By PETER GEACH and MAX BLACK. Oxford: Basil Blackwell, 1952. Pp. x + 244. 25s.

THIS book contains translations of several of Frege's more important articles—*Function und Begriff*, *Ueber Begriff und Gegenstand*, *Ueber Sinn und Bedeutung*, *Was ist eine Function?* and *Die Verneinung*—of his essay on Schroeder's *Algebra der Logik* and of extracts from his review of Husserl's *Philosophie der Arithmetik*, from the *Begriffsschrift* and from the *Grundgesetze*. The publication of such a book is extremely welcome, as it is impossible to form a just idea of Frege's work from the *Grundlagen* alone, and most of these works are now extremely difficult of access. With the inclusion of none of the items can there be any quarrel; in the Husserl review, for instance, Frege gives an important argument, not clearly stated elsewhere, for regarding 0 as a genuine number. It is, however, to be regretted that the editors have omitted the most interesting of the late articles, *Der Gedanke*, and the 1903 articles on Hilbert's *Grundlagen der Geometrie*. The excerpts given from the *Grundgesetze* consist of parts of the Preface, the Introduction, and sections 1-7 of vol. i, and in vol. ii, sections 56-67 and 86-147, and parts of the Appendix.

Readers of the *Grundlagen* will find developed in this volume the distinction between Sinn and Bedeutung, subsequent to that book, and will note Frege's unavailing attempts to extricate himself from the paradox of saying, "The concept 'horse' is not a concept". This dilemma arises because any attempt to say *what* concept "is a horse" stands for must be either a 'saturated' expression, which would stand for an object and not a concept, or an 'unsaturated' one, in which case one could not form a (saturated) *sentence* of the form "'Is a horse' stands for . . .". There are only two ways of escape, neither of which Frege ever took: either one must say that the phrase, "the concept 'horse'" not only does not stand for a concept, but stands for nothing at all, but that the *whole* expression, "The concept '( )' is realised", stands for a second-level concept; or one must deny that the distinction between Sinn and Bedeutung is applicable to other classes of expression than proper names—and indeed it is only for proper names that Frege makes out a good case for the necessity of the distinction. If Frege had taken the former expedient, he would have been forced to set up very much more complicated criteria for deciding whether an expression was a proper name or not, and would have been forced to attend to a question he never discussed—"When is it that one can be sure that a given chunk of a sentence does stand for *something*?". If he had taken the second, the word "concept" would have disappeared from his vocabulary, the use of predicate-letters in quantifiers would have come into question and both the *Grundlagen* and the *Grundgesetze* definitions of individual numbers would have become impossible.

The rigorous distinctions of level—between objects, concepts, second-level concepts, and so on—coincide with the simple theory of types, so far as functions, not classes, are concerned; but Frege introduced extensions of concepts—classes—as objects, and regarded all objects as standing on the same level. (It would, however, be a mistake to think that by introducing classes Frege was necessarily infringing his own stipulations about level: the trouble is rather that their introduction wholesale involves a contradiction about the number of all objects in Frege's logical universe.) The doctrine that all objects are on the same level, taken with Frege's view that a definition of a functional expression must provide a value for it for *whatever* argument it *can* take, leads in *Function and Concept* to the apparently preposterous conclusion that a sense must be given to an expression like "The Sun + 1"; but a passage from the *Grundgesetze* here translated shows how ably Frege could defend even so apparently implausible a result. But, as is well known, the doctrine also led straight to the Russell paradox, and the Appendix to the *Grundgesetze* shows how Frege explicitly rejects the theory of types, and adopts instead a solution which involves that two concepts may have the same extension and yet there be an object—namely the extension itself—falling under one but not under the other. Frege goes on to demonstrate that the distinction between falling under a concept and falling within its extension saves his definitions of numbers from the collapse which this result might have led one to expect; it is a great pity that the editors have omitted this passage, together with all those dealing with class-membership, including his derivation of "If *a* is not the extension of a concept, then *a* falls under that concept if and only if *a* falls within its extension", and of "The extension of a concept does not fall within itself". It would perhaps have been fairer to readers if the editors had here indicated the existence of an article, which Mr. Geach himself told me about, by Sobociński in *Methodos* for 1949, in which it is demonstrated that a contradiction can still be derived in Frege's revised system.

In this volume is translated a section of the *Grundgesetze* criticising the formalists. These are not the same writers as the 'formalists' attacked in the *Grundlagen*, who are dealt with in another section of the *Grundgesetze* also published in this book. In the *Grundlagen*, Frege's target is the 'postulation' of new kinds of number—a procedure which a formalist proper would object to with equal force from his point of view. Here Frege is concerned with the view that the objects of mathematical study are classes of marks on paper, and the rules laid down for writing them in sequence: he suggests that such a theory is incompetent to deal either with number-statements *inside* mathematics ("there are at most two distinct roots of a quadratic equation") or with infinite series.

Also of great interest is Frege's explanation of his view of assertion as (in the words of the *Tractatus*) the only verb of the proposition, and his rejection—on the ground that what we *can* do without we *must* do without—of the idea of denial as a second such verb. Another important point is Frege's insistence that the class (the extension of a concept) cannot be understood—at least as anything relevant to logic—except via the concept.

The translation is in general excellent, being almost everywhere lucid, idiomatic and fluent. The renderings of the technical and quasi-technical terms which Frege used, of which the translators have had the good idea of providing a list, are on the whole the most sensible that can be hit on. I feel that a mistake has been made in deciding usually to translate "Gleichheit" by "equality"; for the latter word would naturally be

taken, except in arithmetical contexts, to *exclude* the sense of 'identity'. The choice is obviously difficult, though, and a clear explanation has been given for what has been done. I think also that "Inhalt" (and "beurtheilbarer Inhalt") might have been added to the glossary; but the matter is not so crucial here as in the *Grundlagen*, where the verb "enthalten", when simply translated "contain", may cause a lot of puzzlement. It might seem that it would have been better to use cognate words to translate "bedeuten" and "Bedeutung": "stand for" is slightly preferable to "refer to", but it has no corresponding abstract noun; this difficulty cannot always be surmounted by periphrasis. It may be that it would ultimately be for the best to employ "mean" and "meaning": it was, after all, possible for Russell to write "the word 'Socrates' ... means a certain man; the word 'mortal' means a certain quality"—and also for Wittgenstein to write "... das Wort 'Bedeutung' sprachwidrig gebraucht wird, wenn man damit das Ding bezeichnet, das dem Wort 'entspricht'".

The book is attractively produced. Both the translation and printing are, however, unfortunately marred by a number of slips. I append a list, which has been checked and agreed to by Mr. Geach, of the seriously misleading ones.

The use of italics, and to some extent of quotation marks, in *Begriffsschrift* and in the Introduction to the *Grundgesetze*, does not always correspond to Frege's. Page 18, line 1: read "content" for "context". Page 22, line 10; "2" for "5". Page 50: remove footnote to page 52, with asterisk after "'the number 4'.", line 30. Page 56, 1st footnote, "in the sense of identity" for "strictly". There is avoidable inconsistency in the translations of the words "Vorstellung", "Bild", "Empfindung" and "Anschauung" in *Ueber Sinn und Bedeutung*, particularly in the footnote on page 59. Page 59, line 4 from foot: "or" for the second occurrence of "of". Page 73, line 3 from foot: "concessive" for "conditional". Page 78, line 6 from foot: "thought" for the second occurrence of "sense". Page 82, line 5: "relates" for "relies". Page 85, line 4 from foot: "say" for "see". There are a number of unnecessary exclamation marks in the article on Schroeder. Page 100, 1st footnote: "95" for "85". Page 106, line 8: after the full stop insert "In this way you do get a domain-calculus, but not a logic". Page 112, lines 6 and 7 from foot: "left" for "right" and *vice versa*. Page 132, line 5: insert after the 2nd full stop: "And this completion is what keeps the whole together." Page 134, line 7: after the full stop insert: "The result of the completion is in each of the two cases a thought." Page 142, line 19: insert "concepts and" before "relations". Page 151, line 17: "because" for "so". Page 152, lines 13 and 14: "which does not stand for a number, as the sign '2' does, but only indeterminately indicates one" for "which ... indefinitely". Page 152, line 9 from foot: delete "In general". Page 154, line 4 from foot: insert "always" before "have". Page 156, lines 5-7 from foot: delete "short straight" and "proceeding ... right". Page 157, footnote: "as if they were names standing for something, although I do not specify what they stand for" for "as names ... reference is". Page 160, line 8 from foot: insert "or symbol" before "stands for". Page 161, line 12 of footnote: insert "of the same symbol" after "conditional definitions". Page 169, line 10 from foot: insert after full stop: "One can try to guard against the dangers arising from this by allowing these letters to indicate, not objects in general (as we have done), but only those of a domain with

precise boundaries." Page 171, line 4: "single" for "simple". Page 174, line 2 from foot: insert after 2nd full stop: "Certain limits to the power of construction ought to be admitted." Page 202, line 18: "what sense these formulas are supposed to have" for "what these . . . mean". Page 202, line 19: "sense" for "reference". Page 204, lines 11 and 15 from foot: for "meaning" read "sense" and "a sense" respectively. Page 205, line 12: read "senseless" for "meaningless". Page 206, line 19: "point" for "sense". Page 212, lines 5 and 6 from foot: read "0 : 0" for "0.0", and insert after full stop: "From these two we could then obtain further the group '3 = 4'." Page 214, line 18: insert after "whether": "an equation like '$0 = \sqrt{2}$' is permissible, then we also do not know whether", and insert "and" before "we" in line 19. Page 215, line 13 from foot: "and" for "to". Page 224, lines 20 and 21: read "(n — 2)th" for "(n — 1)th" and insert at the end of line 20: "terms following the (n — 1)th are smaller than the figure two, and finally: all the". Page 231, footnote: "stand for" for "mean". Page 234, footnote A, line 4: insert "are" after "values". Page 236, line 12: "nine" for "a great multiplicity of". Page 237, line 10: insert "logically" before "compounded".

M. DUMMETT.

Problems of Life. An evaluation of modern biological thought. By Ludwig von Bertalanffy. London: Watts & Co. 1952.

IN this book, Dr. von Bertalanffy expounds the 'organismic conception of life', the formulation of which (with all that it implies) "may well be set beside the great revolutions in human thought" . . . "a second Copernican revolution". "We may hope", says Dr. von Bertalanffy, "that these intellectual developments herald a new epoch which mankind is entering through the formidable crisis of our time." The principal instrument of this revolution, we are repeatedly told, forthrightly or by implication, is Dr. von Bertalanffy himself. Let us now therefore enquire into the credentials of this portentous doctrine.

The organismic conception of biology teaches that organisms are not mere assemblies of their constituent parts; nor are their activities mere agglomerations of discrete reflexes, metabolisms, growings and secretions. Nor again are ecological communities mere jumbles of animals and plants. Quite the contrary: "Every organism represents a *system*, by which we mean a complex of elements in mutual interaction". Among the ideas that are superseded by this illuminating thought are the "classical cell theory", according to which the organism is a mere aggregate of cells; the "classical theory of reflexes", which represents behaviour as a sum or chain of individual reflexes; and the genetical doctrine which affirms that the organism is "an aggregate of characters going back to a corresponding aggregate of genes in the germ cells, transmitted and acting independently of each other". By "sum" and "aggregate" in these contexts, von Bertalanffy expressly means us to understand a *mere* sum or aggregate, one which takes no account of mutual dispositions and functional interrelations, for it is the recognition of these mutual dependencies that constitutes the intellectual triumph of organicism.

The theories and attitudes of mind which the 'organismic conception' supersedes are so conspicuously foolish that one wonders who can have had the temerity to propound them: and so we wait in ghoulish pleasure.

XUM

page after page, for quotations from or references to the authors whose fatuous views they represent. But we wait in vain: expectation gives way to doubt and then at last to the full exactions of hope confounded: there are no such views and no such people, and I submit that it was not necessary to invent them. The "analytical-and-summative mechanist" is merely a sort of lay devil invented to allow nature-philosophers the pleasures of indulging in the rites of exorcism.

Let us now turn to a humbler example of the nature-philosophers' habit of inventing fictitious problems. "A much-discussed problem of histology", von Bertalanffy informs us, is that of "the significance of the intercellular substance". (Intercellular substances—the definite article is uncalled for—include blood plasma, connective tissue fibres, and the matrices of cartilage and bone.)

> "We are presented here with two opposing views. According to the first, these substances must be regarded as dead secretions of the cells, which alone are living; according to the second, the intercellular substance is formed by a transformation of living protoplasm, and the concept of a 'living mass' is maintained which includes not only the cells but also the intercellular substance."

If this is indeed a "much-discussed problem", von Bertalanffy should have had no difficulty in telling us which histologists have discussed it. None have, in my hearing. There is, however, a genuine problem about connective-tissue fibres: are they, like hairs and scurf, made of dead cells, or are they derived from external cellular secretions? Nature-philosophers discuss problems of the first sort, histologists discuss problems of the second sort. Dr. von Bertalanffy may be inclined to say that, in writing so confidently of hairs being composed of dead cells, I have entangled myself in just the profundities that so exercise him. Not a bit of it! The word 'dead' is here used in the good old-fashioned way, that is, to mean 'formerly living'. For laymen seldom speak of stones as being dead, and never of crystals as being in any sense alive; but they can tell a living horse from a dead one, and, what is more, can remember an apt metaphor which turns upon the distinction.

Dr. von Bertalanffy is at great pains to show us what a clever fellow he is; there are many pages of quotations from his own earlier writings, so juxtaposed to citations of the more recent works of others as to convince us of his prescience. But let us be charitable and regard this as part of the literary idiom of nature-philosophy. Of course, the organismic conception is not new, and page 182 contains a formidable list of the names of those who have groped towards it, but von Bertalanffy "believes that he may claim, in his organismic conception developed since 1926, to have first consistently formulated the new attitude as a working hypothesis for biology". The informed reader will find this judgement a little hard upon, for example, Lloyd Morgan and J. S. Haldane (only once confused with J. B. S. Haldane); he will hope that it is an oversight that no dates are attached to citations of their work, though dates are elsewhere the rule; and regret that, perhaps because there are upwards of 50 references to von Bertalanffy's writings in the literature list, no room could be found for references to their own. It is a great pity that the style of von Bertalanffy's book is such as to do him a perhaps irreparable disservice among discriminating readers, for he has some sensible things to say in paragraphs which the reader, exasperated, may easily overlook. Of machines, in the sense of reactively hide-bound

systems with a built-in structural order, von Bertalanffy sensibly says that "Organisms *are not* machines, but they can to a certain extent *become* machines, congeal into machines". He emphasizes too, that entropy is not inconsistent with a "transition towards higher order. For example, crystallization which creates a level of organization higher than that of the molecules, of course follows the law of entropy." This is all good stuff, and, with rather more reserve, the same can be said of his views on the 'historical' character of organisms.

What of the more technical parts of the book? Can the philosopher read this book, if not for its philosophical opinions, at least for an up-to-date and rigorous appraisal of the state of advancement of modern biology? I fear not. The section on embryology is, in my opinion, out of date, for it gives no hint of the misgivings that have troubled the more recent students of organizer theory and takes no account of the powerful testimony of modern microbiology. The chapters on evolution and natural selection are written in a curiously arch tone, and it is not easy to discern what the writer's views really are. It begins with the woeful misconception that "an enormous overproduction of descendants takes place with all organisms" (a misconception only to be arrived at, as R. A. Fisher tells us, "by abolishing a real death rate, while retaining a real rate of reproduction"); it goes on to assure us that selection has been "mathematically proved", a statement to which I can attach no meaning; and somewhat later assures us that "we find no evidence either in the living world of today or of past geological epochs for a continuous transition" between species, a statement that is quite simply erroneous. von Bertalanffy is much concerned with "a fundamental question—though it is usually evaded in the text-books—what the 'gene' or 'hereditary unit' really signifies". Later he tells us in a quotation from his earlier writings that "Genes are the expression of small differences in otherwise corresponding genomes". If this is an answer, it is insufficient and ill-expressed; I recommend von Bertalanffy to consult and master the lucid analyses of Hogben and of Mather.

A more concrete and therefore less excusable example of lack of rigour is to be found in the paragraphs in which von Bertalanffy treats of viruses. Viruses, we learn, "display a property that seems most characteristic of the living: they multiply by division. If, for example, a plant is inoculated with a few hundreds of molecules of crystallized tobacco-mosaic virus . . . an enormous increase of the virus substance" takes place. The second sentence is true; the first, which it purports to exemplify, does not follow from it. I find such an error quite unpardonable.

Dr. von Bertalanffy's omissions are surprising: he has, for example, nothing to say of endocrinology. It is characteristic of endocrinology that it has stood aside from the minor secular shifts of emphasis between analytic and integrative research that are to be found, for example, in the history of the study of animal behaviour. On the contrary, in endocrinology the chemical analysis of hormones and the 'organismic' interpretation of their complex interplay and mutual control have proceeded happily side-by-side, each promoting the interests of the other. The history of endocrinology (and also, less clearly, of immunology, on which he is likewise silent) makes nonsense of von Bertalanffy's quasi-Copernican revolution, and of the slight, implicit in the writings of nature-philosophers, that the laboratory worker ("busy with special questions and experiments") is too preoccupied to have at heart the welfare of the organism as a whole, or of what he calls in a, to my mind, more than

usually odious turn of phrase, the "essential 'vital' features" of living organisms. Perhaps therefore von Bertalanffy was wise to leave out all reference to them.

In a sense von Bertalanffy's book is a prolegomenon to what he calls a General System Theory—"a new realm of science" that will have a "momentous share in future developments". A system can be defined "as a complex of elements standing in interaction", and the subject matter of General System Theory is "a formulation and derivation of those principles which hold for systems in general". It is to be a mathematical theory, appropriately enough, for "mathematics means nothing else than the highest attainable form of rationalization of reality". Unfortunately, there is nothing in the present work about General System Theory except immoderate claims for its efficacy: we are reminded of that convention of the cinema by which attractions yet to come are heralded by a prospectus that turns the stomach of discriminating filmgoers. But let there be no mistake about it: if von Bertalanffy does indeed formulate a General System Theory, it may well be a work of real importance; let us only beg that, when it arrives, its merits will be allowed to speak for themselves and not be condemned by their author's intemperate advocacy. For von Bertalanffy is a genuine, if never a humble seeker after truth; he has devoted his professional life to the investigation of problems of which, if some are spurious and others the outcome of confused thinking, others still are of the most serious import; if he will be content to produce a well-finished, rigorous and exhaustive analysis of some one problem whose solution lays claim to something less than a universal ambit of significance, he will earn the gratitude and respect of biologists instead of an exasperation that can only lead to neglect and contempt.

P. B. MEDAWAR.

Bertrand Russell's Construction of the External World. By CHARLES A. FRITZ, Jnr. London: Routledge and Kegan Paul, 1952. Pp. 243. £1 3s.

THIS book contains, after some introductory matter, first an account of Russell's definitions of number and class, and his analysis of descriptions, as paradigm cases of the employment of the idea of logical constructions, next a more detailed critical examination of Russell's use of logical constructions in his account of the external world and the entities of physics, and finally a consideration of Russell's methodology, with special reference to the theory of logical constructions.

With reservations to follow, I think Mr. Fritz gives an accurate and well-balanced account of Russell's main logical, epistemological and metaphysical theories. His criticisms of these theories are clear, sensible, and on points of importance; Mr. Fritz is in reasonably close sympathy with Russell, and never challenges his basic conceptions about the nature of philosophy, but he sternly criticises the consistency and validity of Russell's argumentation.

Surprisingly enough, the weakness of Mr. Fritz as an expositor is mainly in regard to the theory of logical constructions itself, which he wishes especially to emphasize. On this topic his terminology is even slacker than Russell's. Thus Chapter ii is entitled 'The Construction of Numbers, Descriptions and Classes'; on page 154 we read: "It is at

this point that the analysis of 'material object' as a logical construction, as an incomplete symbol, becomes significant"; and on page 215 we are told that "numbers, descriptions, and material objects, for example, are 'incomplete symbols'." Mr. Fritz does not seem to realize that only expressions, such as 'material object', or descriptions, can be said to be incomplete symbols; material objects and the King of France are logical constructions, and are not symbols at all, complete or incomplete. It is perhaps this terminological slackness which makes him fail to see such points as that it is a very different thing to say that 'The sense-datum I now have' is an incomplete symbol (because it does not name the nameable) and to say that 'material object' is an incomplete symbol (because it does not denote anything which could be named).

Philosophically, Mr. Fritz writes in a thoroughly intelligible manner; but he is much better at logical, than at English, syntax. At the bottom of the second page of the first chapter we read: "The general philosophic view which Russell had at this time was a rather extreme realism, believing in the reality of many kinds of entities, especially physical objects, minds and universals." This is at least the fourth unconstruable sentence in the chapter. This is a point on which he might have modelled himself on the subject of his examination.

This is not a final account of Russell's philosophical achievement; but in spite of its glaring inaccuracies, and partly because these inaccuracies, when they occur, are so glaring, there is sufficient sound exegesis and sane criticism to make it worth reading by the many students of Russell's philosophy.

J. O. URMSON.

The Origin and Goal of History. By KARL JASPERS. Translated by MICHAEL BULLOCK. Routledge and Kegan Paul. 21s.

PROFESSOR JASPERS holds as an article of faith that "mankind has one single origin and one goal". He confesses to total ignorance of both, and seems not to believe that his creed is literally true. However, it forms the ostensible topic of his lengthy and arduous book, most of which consists in a survey of the history of the world.

This survey can be briefly summarized. Of prehistory we know, it is said, almost nothing at all. The first uses of fire and of tools were affairs of importance, but the pioneers in this field have left no memorial. The most ancient civilizations of which we have knowledge are also, though not entirely opaque to historians, yet "destitute of any specifically historical movement". The first excitements of history are to be found in the "Axial Period", from 800 to 200 B.C. In these centuries there occurred events more striking and important than any before or since. Then the West, the chief legatee of Axial energy, began in the sixteenth century to take significant steps on its own account. But Jaspers goes on to warn us that Science, the prestige of which is now high, is yet not able to answer all our questions; and that Technology, which has indeed revolutionized Life, may be a force for ill as well as for good. With the growth of facilities for transport, the World has become as it were a smaller place; and this is in some ways saddening, though in other ways good. The Future he finds impossible wholly to foresee. Bureaucracy is likely to increase and may degenerate: Planning is much in vogue but has limitations: a World Empire may possibly come into being by

conquest, or perhaps a World Unity by agreement among nations. What is needed to arm us against these or other contingencies is faith in God, in Man, and in " possibilities in the world ". The aim which in some way makes of History a Unity is to achieve in all men " the manifestation of Being ". In the present, however, the most important problems are still those that require us to decide what actually to do.

One is likely to experience a sense of tedium in considering this summary of Jasper's volume. It is indeed, like the content of his argument, platitudinous. The views on universal history which he presents are not, so far as they are intelligible, in general surprising ; nor are they liable to criticism in detail, for they contain no detail. His statements are about Science and Man, Civilization and Society ; for the most part their truth or falsehood is a matter of choice, with no great incentive to choose. Why then is the book so large, and its potential public, perhaps, no less large and solemn ?

The answer to this enigma no doubt lies in the language. The translation, which must have been very difficult to make, is also difficult to read, being needlessly barbarous and verbally ugly. But it can be read, not without a certain suspension of judgment, in such a way that it begins to act in the manner of a drug. The contorted verbosity of Jasper's sentences may convey the vague excitement of confused shouting, or enfold the reader as if in a cloud of mildly intoxicating vapour. In certain stages of intoxication quite unremarkable reflections may seem for a moment to offer the key to the Universe ; life seems to be seen, not steadily, but vividly, in depth. This book will be of no use at all to readers not liable to verbal inebriation ; there are also more pleasing ways than this of achieving that condition. But those (and no doubt they are many) who can drink these draughts with pleasure will probably feel, for some moments at least, that at last the canvas of Man's history has been surveyed by a powerful and a penetrating eye. When they read that " the multiple is subject to the imperative of the One ", and that " this is the exclusivity of the demand of an historicity to be the only one and to dominate all others, but it must develop for consciousness in the communication of the multiple historical in the absolute historicity of the One ", it is not impossible that they may feel that some secret is about to be, or even that it has been, laid bare.

G. J. WARNOCK.

Science, Language and Human Rights. Philadelphia ; University of Pennsylvania Press, 1952. Pp. 207. $2.50.

THIS volume consists of the papers read at the annual meeting of the Eastern Division of the American Philosophical Association in December 1952. There are five symposia, each consisting of two papers. The general level of both competence and seriousness is markedly high, and it is to be hoped that philosophers on this side of the Atlantic will give this book the attention it deserves.

The first discussion is between Roderick Firth and Max Black, who contrive to make interesting and original contributions to the well-worn topic of Phenomenalism. Firth sets himself the task of estimating the comparative merits of the sense datum language and what he calls the " language of appearing ", which is the notation advocated by those philosophers who prefer colloquial expressions like " looks ", " seems '

"feels", "appears", to technical talk about sense data. Firth concludes, after an elaborate and careful discussion which cannot be summarized here, that for "epistemological purposes" the language of appearing is inadequate since it does not permit us to describe hallucinations. He points out that although it would be perfectly possible to assign a phenomenological meaning to phrases like "It appears to me that . . .", this would amount to saying that there is some "sensous aspect or component" common to both illusory and veridical sense experiences, and this of course is nothing other than our old friend the sense datum once again. Firth does not defend the sense datum terminology except in this negative way, while Black does not defend the language of appearing but criticizes the sense datum language, hence their discussion is not altogether satisfactory considered as a piece of philosophical dialectic. Black adopts the Wittgensteinian method of exposing the limitations of the sense datum language by considering the question of how such a language might be taught. He concludes, I think correctly, that if sense datum propositions are incorrigible, then no logical relations can properly be said to obtain between them, for nothing that one person says in this language can count either in favour of or against anything that another person says in the same notation. This shows, says Black, that the sense datum language is futile as a means of communication and hence not a language at all in any legitimate sense of the word. Black could have reinforced this conclusion by making two further observations. (i) The expressions used by phenomenalists to refer to "immediate experience" have got what meaning they have only by courtesy, so to speak, of the allegedly logically independent "material object language". That is, we can attach significance to the phrase "red patch" in statements like "I now have a sense datum of a red patch" only because we know how to use the phrase in describing things like bloodstains and the faces of clowns. (ii) The mythological character of a language which describes only "the given" is also clear from the fact (which I think is ultimately a logical fact, if one may use such an expression) that to be in a position to say something is also to be in a position to be able to make a mistake. This was often pointed out by nineteenth-century idealists, though in an idiom now most unfashionable.

The question "What is a Rule of Language?" is debated between N. L. Wilson and R. M. Martin. Their discussion is based on, and in my view vitiated by, the very odd assumption that whatever is true of an artificial language will also be true of a natural language like Spanish. Wilson says "It is sheerly a matter of accident that some languages are used by people, others are defined in such books as Carnap's *Semantics* and still others, the vast majority, enjoy neither of these benefits". Waiving the problem of exactly what sort of existence these mysterious unspoken and unformulated languages could have, it is plain that Wilson is here forgetting that when we refer to a calculus as a "language" we are speaking in a highly metaphorical way. It is therefore very much an open question to what extent the construction of artificial systems or "logically perfect languages" can throw light on philosophical difficulties, which arise in the notoriously "imperfect" natural languages.

The other contributors to this volume are as follows: E. Nagel and C. G. Hempel, *Problems of Concept and Theory Formation in the Social Sciences;* V. A. Tomas and D. N. Morgan, *The Concept of Expression in Art;* A. I. Melden and W. K. Frankena, *The Concept of Universal Human Rights.*

These topics have on occasion induced a kind of hypnosis in those who investigate them, and a peculiarly unrewarding type of sententious woolliness has been the result. These philosophers, however, are not in the least mesmerised, and on the whole deal with their problems with both care and insight. The last mentioned discussion, in particular, is a stimulating and original contribution to a problem to which philosophers have recently paid insufficient attention. In short, this is a book which may be commended without hesitation.

C. K. GRANT.

Christianity in an Age of Science. By C. A. COULSON.

Languages, Standpoints, and Attitudes. By H. A. HODGES. Oxford University Press, 1953. Riddell Memorial Lectures. Paper. Pp. 53 and 68. 5s. each.

PROFESSOR COULSON rejects any "Parcelling out of the country of the mind into departments under independent authority" (p. 10) treating "religion and science as if they were two adjoining allotments, each to be separately cultivated". Instead he would like to say that scientists and theologians are pursuing different "lines of inquiry into the nature of reality" (p. 35) and are all "describing the same situation . . . though each uses his own appropriate language" (p. 45). "We can give full weight to the Genesis story of creation and at the same time admit without any embarrassment whatever the biological account of evolution" (p. 22). So we can—though this is not explained—by interpreting the story of, say, the Fall as a myth; which expresses facts of one category, the perennially sinful propensities of mankind, in the idiom appropriate to those of another, history. This spares us the doctrine of original sin "as the result of a sin committed in actual historical fact, by an individual man named Adam . . . native to all of us only because it has been handed down by descent from him" (Pope Pius XII in the encyclical *Humani Generis*, 1950).

Again in a great decision "the one unchallengeable fact for the President is his own free will, and the grim necessity to make up his mind" (p. 23), whereas afterwards to a historian the upshot is "quite inevitable"; a matter of the difference between actor-language and spectator-language (Kant's contrast between freedom in the noumenal world and determinism in the phenomenal might similarly be reinterpreted as one between the 'languages' of morals and of neutral observation).

While to speak of different languages in our 'One World' is an advance on the older fashion of demarcating bailiwicks and multiplying peculiar worlds: saying things about the same subject is sometimes confused with saying the same thing. We are told that the scientists reporting "Racial discrimination has no basis in biological fact" were saying "in their language" that "there is neither Jew nor Greek, bond nor free, but all are one in Christ" (p. 45). But for St. Paul the reference to Christ is not a literary trapping, irrelevant to the sense.

By contrast Professor Hodges, in a rather abstract study of fundamental issues nowadays too often eschewed by professionals, insists "It is just not true that, given sufficient ingenuity, all standpoints can be brought into harmony" (p. 44). A *standpoint* is a "set of principles or presuppositions together with the type of question to which they give rise" (p. 15) and it "tends to find expression in a characteristic language"

(p. 16). He is concerned to stress that "the philosophical theory of language" cannot grade or dismiss any of these any more than "the theory of knowledge can dismiss a particular field as illusory" (p. 22). Perhaps this is *strictly* true: but such work as Martin's 'A Religious Way of Knowing' (MIND, 1952) might suggest to some that sometimes *either* the analogy with other cases of knowing is very weak *or* what was known was not after all God the Creator. As a criterion between standpoints: "If there is a standpoint which is able to make use of, and stimulate purposeful enquiries within, a world of discourse which a rival standpoint can only dismiss as meaningless or at least as mere subjective fantasy, this difference must be recognized as a decisive point in favour of the former standpoint" (p. 64). This is surely arbitrarily to stack the cards against denial that religious experience or revelation really discloses the Management of the Universe.

ANTONY FLEW.

The Hierarchy of Heaven and Earth. By D. E. HARDING. Faber & Faber. 21s.

THIS book is an attack on certain prevalent dramatisations of the universe. Putting it crudely, it resists the view that, since the universe is no longer For us, it must be Against us. It examines the situation of man, and stresses the points in which he is continuous with, rather than opposed to, the rest of physical matter. This might place it with a welter of neo-this-and-that apologias, made futile by their failure to grasp the modern thought which they try to attack. From these Mr. Harding must be clearly distinguished. First, he is not in the least apologetic, not at all concerned with straining cold bath-water for babies who grew up long ago. Second, he is free of conventional jargon. (He has some private jargon of his own, not fully justified, but not very damaging.) He is well up both in modern philosophy (particularly, and crucially, the theory of types) and in relativity theory, but avoids the technical language of either. His firm refusal to have his subject classified under any existing head justifies, needs, and usually gets clear forceful Ordinary Language. This has three results which will probably be valued even by philosophers who mistrust his more positive views. First, many shrewd attacks on the dramatisation I have mentioned, which is certainly very influential and not fully recognised—belief in Man as the heroic and persecuted antagonist of the Second Law of Thermodynamics, hurling defiance at a sadistic universe, without which, one may remark, he would certainly be a great deal worse off than he is now. Second, an educative non-Cartesian restatement *ab ovo* of the human situation. What am I? A human being like others. But I haven't got a head, which is a difference. What makes me think I have? Where is it? etc. The antithesis of matter and spirit is neatly dissolved ("My mind is the view out from this centre, my body the view in") and puzzles such as "where are my thoughts?" are tidied up effectively. It is all very ingenious, and would certainly make a change for the intelligent pupil. Third, an account, equally simple and penetrating, of Time and Space puzzles, with some good attacks on the Fear of Eternity.

Beyond these points, Mr. Harding gets more controversial. He illustrates the suitability of man to Nature, and the general harmony of the cosmos, with a dashing structure of paradoxes and analogies. (The universe, it turns out, *is* on our side after all, but in a new way.) Some of

his arguments are old. Yet I find them much improved by being restated, without idealist pomposity or Platonic puritanism, in this incisive, forceful, personal style, which is often extremely beautiful. The intention of the book is religious, but it never relies on authority. Its poetry is certainly neither foolish, dishonest nor tendencious. It *might* be mad, but I think in an extremely useful way.

MARY SCRUTTON.

Descartes and the Modern Mind. By A. G. A. BALZ. Yale University Press. London: Geoffrey Cumberlege, 1952. Pp. xiv + 492. 63s.

THIS book is particularly difficult to review because, though not without merits, it leaves the reader with a vague impression hardly capable of summarization and is lacking in salient points of a type that could be stated briefly. The author has, I think, much learning and some philosophical ability and has pondered much on the subject. His method of approach is in a way original, especially in his working out of the antithesis between reason in man and man as a human personality, which he expresses rather quaintly by heading one main part of his book *Cartesius* and the other *René Descartes*. But I must say that I doubt whether the work has made any important contribution to the subject for experts and whether, if used as a text-book for students, it would not confuse more than illuminate. Certainly I am unable to recommend it as conveying anything that is worth the very high price charged (3 guineas).

In style the book is far too prolix. It seems to me that what has been said could have been better expressed at much less than half the length and so presumably at less than half the price (an important consideration nowadays). There is a great deal of rambling discussion from which it is hard to derive a clear definite impression, and in places the style becomes very obscure, especially when the author plunges into metaphysics of a kind that does, even in my opinion, deserve the strictures of the positivists. Incidentally, if anything could convert me to logical positivism, it would be a really prolonged course in the sort of metaphysics which the author connects with the ontological proof. It is only fair to add, however, that the metaphysical passages to which I am referring only make up a small part of the book.

The title, *Descartes and the Modern Mind*, is very misleading. It leads one to expect a comprehensive account of the relation of Descartes to the modern outlook in general, and such a work is needed and might have been very valuable. But the author seems to have far too little acquaintance with modern developments of thought to write such an account, being apparently not familiar even with the recent logical criticisms of the ontological proof, and he does little to bring out the importance of Descartes as the father of what is commonly known as the scientific outlook. The general remarks he makes about science, though no doubt substantially true, seem to have very little connection with Descartes, referring as they do mainly to that aspect of modern science in which it differs most from his spirit, I mean its tentative and problematic character, and doing little to bring out his real influence on the development of the mechanistic outlook and on the actual methods of science.

A. C. EWING.

Principium Sapientiae: A Study of the Origins of Greek Philosophical Thought. By F. M. CORNFORD. Edited by W. K. C. GUTHRIE. Cambridge: Cambridge University Press, 1952. Pp. x+270. 25s.

PROFESSOR GUTHRIE has prepared the present work for publication, and has contributed a most useful Appendix, which provides the summing up Cornford did not live to complete and helps to some extent to draw together the two parts into which the book is divided. In the first part, which has the general title "Empiricism versus Inspiration", Cornford begins by emphasising the general neglect of a genuinely scientific method of observation and experiment in the early Greek philosophers, a neglect which is reflected in Epicurean atomism, the final outcome of Ionian physics—for example Epicurus advances his central thesis about atoms and void not as a tentative hypothesis, which at best must remain dubious in the absence of experimental confirmation, but as something known to be true by an act of "mental apprehension", by a "projection of the mind". It is in the medical writers of the Hippocratic corpus that an empirical theory of knowledge is to be found. Epicurus, like "all the other philosophers", was dominated by a totally different theory, "a belief in the power of the mind to project itself outside the body and apprehend wisdom which can never be revealed through sense". This too, as Cornford seeks to show by reference to the *Meno*, *Phaedo*, *Symposium*, *Republic*, and *Phaedrus*, was a characteristically Platonic view, a view inherited by the philosophers from the much older tradition of the *shamans*, who united in one person the characters of prophet, poet, and sage. Thus the philosopher is the successor of the seer-poet.

Throughout this section of the book there is much to interest the student of Greek philosophy, and, particularly in Chapters IV and V, the student of Plato; there are some things too which may not carry conviction. For example, in the search for the origins of Greek philosophy a dichotomy seems to develop such that we are faced with the choice, either men of science or poet-seers. But it might perhaps be argued that philosophy originates where neither of these categories holds, and in this connexion Cornford's treatment of Socrates, "the ironic dialectician", who does not seem to fit into either category, merits particular attention.

The second part of the book is perhaps of primary interest to the specialist in anthropology and primitive religion. Here, after reconstructing Anaximander's system as typical of Ionian cosmogonies, Cornford relates it to Hesiod's cosmogony, pointing out that Anaximander's system is not, as it might appear, a rational construction based on direct observation of the existing world, but that he is working on a scheme already provided by Hesiod. He then examines the mythical account of the origin of the world which follows and duplicates the semi-philosophical account in Hesiod's Theogony, and seeks to show the oriental and ultimately ritual origin of certain of the myths in the Theogony.

The book is finely produced, and forms a fitting reminder of the width and depth of Cornford's scholarship.

R. C. CROSS.

The Philosophy of Aristotle. By D. J. ALLAN. Home University Library. London: Geoffrey Cumberlege, Oxford U.P., 1952. Pp. 220.

MR. ALLAN has here tried to give in about 57,000 words an introductory account of Aristotle's philosophy and a criticism of its value. He is not very successful, because his criticism is apologetic or moralistic, and because he cannot command the style of general exposition. Those who desire to get a general idea of Aristotle's thought without studying his works will do better to read either the shorter account in W. K. C. Guthrie's *Greek Philosophers from Thales to Aristotle*, or the longer account in Sir David Ross's *Aristotle*.

Mr. Allan never begins with one of Aristotle's principles and goes on from there. Nearly all of his fifteen chapters start instead either with some modern criticism of Aristotle, or with Plato's views on the subject, or with literary information about the meaning of a Greek word or the nature of one of Aristotle's surviving works. The first two chapters are wholly devoted to literary and historical topics, namely the form of Aristotle's writings and the growth of his philosophy. Mr. Allan thinks that by beginning thus he is making 'the path smoother for those who approach this study without a classical training'; but this is not so if he means the path to a general elementary knowledge of Aristotle's mature philosophy.

I illustrate the disappointing and bewildering nature of his exposition from the chapter called 'Formal Logic'. Here we start with the question of the place of logic among the sciences, which Aristotle himself never discussed. After a page and a half of that we come to what we want, an exposition of Aristotle's logical doctrines. But after only a page and a half of that, and in the middle of a paragraph, we switch to literary topics, namely the contents and dates of the various logical works. When that is finished, Mr. Allan reports and controverts a view of Stebbing's about Aristotle's logic, although he has not yet told us enough about Aristotle's logic for us to have any idea of the relevance or truth of Stebbing's view. He criticizes Aristotle's account of propositions before he tells us what it is. Then he seems to be going to tell us what it is: 'We must not, however, ignore Aristotle's account of propositions, such as it is'. But before we can have it we have to read a paragraph on Plato's account of propositions, a paragraph which does not help the subsequent exposition of Aristotle. Throughout the book the reader has the uneasy feeling that Mr. Allan does not know where he is going next. The order of topics is never obvious.

In other ways also the exposition is often bewildering. On pages 152-153 he seems to leave the impression that he is a Josephian believer in essence, but on page 112 he had flatly rejected essence. On page 183 a short paragraph starts by implying that Aristotle did not wish the young to be educated by the state, and ends by implying that he did. On page 193 we read: 'A number of questions arise from this account of political society. Firstly . . . Secondly . . . Thirdly . . . Finally . . .'. But none of the four matters thus introduced is a question.

The doctrines of Aristotle and his critics are frequently judged, and the judgment has frequently a moralistic tone: an error is a 'fault', or a 'serious fault', or a 'grave fault'. But really there is little need to condemn Aristotle for 'faults', or to 'defend' him against 'unfair blame'. The reader will form his own opinion on all that. What he

wants from the writer is to know what Aristotle said; and if Mr. Allan had omitted the judgments he would not have had to say that he 'must pass over' this or that interesting doctrine or argument of Aristotle's (*e.g.* 117).

Mr. Allan's literary character is rather like A. E. Taylor's, apologetic, partisan, moralizing, earnest, bewildering, learned, detailed, imposing. His strength is not in generalization, but in the destruction of generalizations by new and unexpected details. He sees things as more complicated, and less capable of summary statement, than most people think. He is aware of many historical connexions. His book can be recommended more to those who have studied Aristotle than to those who have not. They will find in it many interesting insights and suggestions, although nearly all of them, owing to the abortive intention to make this an introductory book for beginners, are too briefly put.

RICHARD ROBINSON.

Mimesis: Plato's doctrine of artistic imitation and its meaning for us. By W. J. VERDENIUS. Leiden: E. J. Brill, 1949. Pp. 50. 2.30 guilders.

IN this monograph Professor Verdenius expounds Plato's view of artistic imitation, and defends the theses that a general aesthetic theory may be of assistance in the appreciation of works of art, and that if interpretation is a feature of art, there is a place for Plato's concept of *mimesis* beside the notions of expression and creation. The author starts from those passages in which Plato says that the poet is inspired or possessed, and can give no rational account of his work. This might be expected to lead to the conclusion that he is infallible in so far as he is truly possessed, but Plato proceeds to consider him also as an imitator and to compare him unfavourably with the craftsman. The fact is, then, that the Muse leaves it to the inspired man to interpret his vision, in doing which he must make use of imitation. From the very nature of imitation his work cannot be adequate to an external object, as Plato showed in the *Cratylus cf.* Collingwood in MIND, vol. xxxiv (1925); but there is a sense in which it may be superior; for imitation can represent things higher than those which are met with in daily life (*Rep.* 401c and 472d).

It is interesting to find the view that the poet is a mere imitator linked up with, and subordinated to, the view that he is inspired. I am not convinced that these views occur side by side in the same dialogues; and feel some doubt, which this essay has not dispelled, about the consistency of Plato's use of the term *mimesis*. In *Rep.* Book X he continues to address his remarks to a supposed imitative class of poets, while pointing out defects which must belong to poetry as such. Those who have held that *mimesis* comes in only when Plato is considering the subject from the educational point of view (and that his concept, if it has permanent value, can have it only *per accidens*) seem to be on safer ground.

There are full bibliographical references which compensate for the brief discussion of various disputed points.

D. J. ALLAN.

Directio Speculantis seu de Non Aliud. By NICOLAS OF CUSA. Edited by L. BAUR and P. WILPERT (vol. xiii of the *Opera Omnia* edited by the Heidelberg Academy). Leipzig, Felix Meiner, 1944 but issued in 1953. Pp. xx + 72 and 3 pp. of Addenda et Corrigenda. 28 German Marks.

IN this treatise written in 1462 Nicolas of Cusa tries to demolish the Aristotelian doctrine of definition, and proclaims a revolutionary and, in his view, healing formula for definitions which "make us know things". The Aristotelian formula 'A is B' is equivalent to 'something is something else' and, therefore, wrong: had Aristotle realized this, he would have saved himself and us much trouble. Platonic and quasi-Platonic formulae like 'A is A', 'A is itself', 'A is one' are, in this order, somehow nearer and nearer to the truth, but still uninformative. Nicolas's alternative is 'A is nothing else (*non aliud*) but A': it is informative because it excludes from A everything else. Even more, the phrase 'non aliud' expresses the essential self-identity of the definiendum. Above all, 'non aliud' is, in definitions, a reminder that things receive their being, reality, self-identity, from *the* Non-Aliud: God, the unnameable, who can only be described by means of 'non- . . .' labels, can be best called 'Non-Aliud'. His more indelible mark on things is their non-otherness; by this term they are known, because by Non-Otherness they are made: "principium cognoscendi idem est ac principium essendi."

This doctrine is of interest for the historian of Platonism: the convergence of negative theology, mystical annihilation, relentless criticism of logical formulae appears to have stultified all attempts at a positive knowledge not only of God but of our more modest things; it is the culmination of a long process ending in a total denial. Others will find in Nicolas some support against the 'A'–A correspondence theory, especially if applied to predicates. Monopolists of philosophical panaceas distilled into one word or phrase could do worse than consider how the desolate *non-aliud* was made into the best possible *directio speculantis*.

The text as now printed is superior to that of the only previous edition: Wilpert realized that the one existing manuscript was copied from a volume where a leaf had been misplaced; and a few other mistakes have been corrected. Apparatus of sources and readings, index, and two prefaces written in Pidgin Latin add unequally to the value of this beautifully printed fascicule. Omissions and mistakes found after 1944 are put right on a separate leaf.

L. MINIO-PALUELLO.

St. Thomas and the Existence of God: Three Interpretations. By WILLIAM BRYAR. Henry Regnery Company, Chicago, 1951. $5.00.

MIND's review of E. Gilson's *L'être et l'essence* spoke of the need for a prodigy who could "combine M. Gilson's magnificent grasp of philosophical tradition with a command of the technical resources of modern logic", and at first sight it would appear that Mr. Bryar is attempting to supply this need, at least in a restricted field. References to the work not only of Gilson, but also to that of Maréchal, Adler, Chenu and Maritain mingle with the names of logicians such as Bochenski, Salamucha and Carnap. The aims of the book are at the outset encouragingly

modest, in that it purports to be a non-polemical inspection of the language used by St. Thomas Aquinas in the *prima via* of I, q. 2 a. 3 of the *Summa Theologica:* this text is that of the proof of the existence of God from motion. Elucidation is not immediately sought from parallel passages on the same subject; rather the author claims to lay bare what Gilson has called the "prescientific option" which lies at the root of St. Thomas' metaphysics. "A tentative reconstruction of the process through which St. Thomas would have passed to arrive at the place where the proof begins" (p. xviii) is the aim, yet it is admitted that in the last resort, "the description will only be of value in the measure in which its means can be indirectly justified by a sufficient number and accurate study of texts of the *Summa*" (*ibid.*).

Three possible levels of interpretation are then developed at great length in a "Systematic Interpretative Study", the first "Division" of which deals with the "existentiality" of things, the second with what is called the "partiality" of things, and the third with the "contentuality" of things. In view of the criteria of evaluation mentioned, it is scarcely surprising that the outcome consists, in effect, of sophisticated yet orthodox thomist analyses of the notions "*ens*", "*res*" and "*aliquid*". That this manner of working *really* "avoids that mode of interpretation which proceeds by way of a greater generality and would work back from a complete 'reflective' understanding of St. Thomas as a whole and some subsuming general rules to explicate a particular case" (p. 133) is therefore hard to understand: clearly, any discrepancy with the texts would have led to revisions in the course of the composition of the analyses. However, the proof is scrutinised in the light of these three lines of interpretation which are assumed to be "naturally prior in the logical structure of the growth of knowledge for St. Thomas" (p. 132).

Penetrating and complete though much of this study may be, the turgid style and unwieldy arrangement make it very painful reading: pages of cryptic paragraphs *à la* Wittgenstein, a profusion of repetitive prefaces, preambles, cross-classifications and unincisive jargon, the continued recurrence of the future tense in the body of the work, coupled with the retrospective character of the weighty appendices, all combine to blind the reader to the precise geography of the subject under discussion. The appendices should be read first, as the salient points are there explained in terms of the much more intelligible writings of Gilson and others.

D. P. HENRY.

Freedom: A New Analysis. By MAURICE CRANSTON. London: Longmans Green & Co., 1953. Pp. viii + 177. 12s. 6d.

THIS book is divided into three very distinct parts. Part I contains Mr. Cranston's general account of freedom together with analyses of various other accounts which have been given of it. He maintains that 'freedom' means 'the absence of constraints', and hence, that there are as many kinds of freedom as there are varieties of constraint. This account, Mr. Cranston claims, is a lexicographical definition, as were the accounts of some other writers on freedom, *e.g.* that of Hobbes; it differs from these alternative lexicographical definitions in being the true one. Mr. Cranston goes on to show that still other writers have provided stipulative or persuasive definitions of 'freedom'. With considerable

skill he uses these accounts to illustrate his own thesis that the issue of 'freedom versus tyranny' is a constantly changing issue, different constraints being felt as particularly irksome at different stages of social development.

Part II, a study of liberalism, illustrates the same thesis in a different way.

Part III, which aims at being a new treatment of the free will problem, contains some interesting philosophical discussions; and it is therefore a matter for regret that space is devoted to a preliminary and quite superfluous review of the classical writings on the subject. When the great have been once more entombed Mr. Cranston has to show that recent writers have been wrong in supposing that the problem should be buried with them, the ground of his objection to its interment being that the real issue, namely, the predictability of human actions, remains undecided. With a view to settling this question he divides predictions into 'predictions about you', 'predictions about yourself', and 'predictions about other people'. Predictions of the first two varieties are seen to be impossible since they require to be infinitely extended to take account of their own repercussions on those whose behaviour they seek to predict. Predictions about other people also fall short of the determinist's mark, for it is impossible, in principle, to predict inventions or creations. The argument of Part III seems to require supplementation at a number of points even if it is valid. First, more should have been said to show that the issue at stake is predictability; secondly, a closer analysis is required of the nature of the dispute concerning predictability; and thirdly, some attempt should have been made to show that such freedom as the argument establishes (if it is valid) is of ethical importance.

H. J. N. HORSBURGH.

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VIII.—NOTES

CENTRE FOR THE PHILOSOPHY OF SCIENCE

A grant by the Louis W. & Maud Hill Foundation of St. Paul has made possible the formation of the *Minnesota Centre for Philosophy of Science*, which is administratively a department of the University of Minnesota in Minneapolis. The present research staff includes: Professor Herbert Feigl, director; Professor Wilfrid Sellars (Chairman, Philosophy Department); Professor Paul Meehl (Chairman, Psychology Department); and Michael Scriven (M.A., Melbourne, Instructor in Philosophy).

Research on the logical and methodological structure of psychology will be undertaken this year; possible areas for future study include the foundations of probability and induction, and conceptual problems of physics, biology and the social sciences. There will be a number of small conferences to which scholars from other universities will be invited.

SECOND SOUTH AFRICAN CONGRESS FOR THE ADVANCEMENT OF PHILOSOPHY

The second S.A. Congress for the advancement of philosophy (Pretoria, 4th-8th May), was attended by 70 participants from all parts of the country and representing all the universities. The Principal of the Pretoria University, Professor dr C. H. Rautenbach acted as Honorary Chairman; Professor dr C. K. Oberholzer and Professor dr H. J. de Vleeschauwer were chairman and secretary.

The Congress was opened by the Hon. Chairman. Different subjects were presented: the philosophy of values, the philosophy of existence, logic and epistemology and the history of Protestant philosophy. In the first group, papers were read by Professor dr Oberholzer (Pretoria): "The Place of the philosophy of values in the System of philosophy; by Dr. Degenaar (Stellenbosch): "About Humanism"; by Dr. Versfeld (Capetown): "Existence in the philosophy of Aquinas"; by Dr. du Plessis

(Capetown): "Heidegger's Rejection of philosophy". In the Logic group Dr. Meier (Pretoria) dealt with Semantic and epistemology, Professor Harris (Johannesburg) with Some linguistic Analysis, Dr. Yourgrau (Johannesburg) with Scientific and Philosophical Constants and Professor dr Malan (Johannesburg) with Basical Logic. In the group "History": 'The outset of Protestant philosophy in the sixteenth century", by Professor dr H. J. de Vleeschauwer (University of South Africa); "Deflections in the protestant Thought during the eighteenth and nineteenth centuries", by Professor dr Venter (Bloemfontein) and "The Fundamental Principles of the Calvinist Philosophy", by Professor dr Stoker (Potchefstroom).

Professor Dr. Murray (Capetown) spoke about "A Liberal Philosophy for a racially plural Country" and Professor Dr. Hugo (Pretoria) about "Some failures in democracy".

The medium of the Congress was Afrikaans and English. Congresses are to be held every two years, the third Congress will be at Stellenbosch in 1955. Address of the Congress—Professor dr H. J. de Vleeschauwer, University of South Africa, P.O. Box 1513, Pretoria.

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